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PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Anæsthesia as an effect of Brain-disease.*[*] By CHARLES
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INTRODUCTION.

MR. PRESIDENT AND GENTLEMEN,—It is a long time now since
I had the honour of lecturing to an audience in Dublin; and I shall
always remember the warm welcome you gave me once in this
College, and before that, in 1859, in another place. The subject
on which I am now about to lecture is extremely vast; and, there-
fore, I ask your attention on'account of the multiplicity of facts
that I shall have to mention rapidly.

You know already that I am to speak of anæsthesia, amaurosis,
and aphasia, as the effects of brain-disease. This subject, vast as
it is, is only a small part of a very much larger one, on which I
have already delivered three lectures in the College of Physicians
of London, and upon which I intend, if I can do so, to deliver
some twenty or thirty lectures more in various places. The
general subject embraces all that relates to the physiology and
pathology of the brain, and comprehends, as a subordinate branch,
the origin of the symptoms of brain-disease.

It is essential before I speak of anæsthesia—which to-day and in

[*] A Discourse delivered before the President and Fellows of the King and Queen's
College of Physicians, Thursday, November 23, 1876.

my next lecture will be my principal subject—that I should give some introductory remarks upon views which I consider extremely wrong, which I was taught when a young man, and against which, I must say, I rebelled for a long time. It is essential, also, that I should at once give an outline of views which I consider to be absolutely new, and which I hope to show you are as true and correct as they are new.

You are well aware that the nervous system is considered to be double, one half of it occupying the right side of the cranium and the left side of the body, and the other half the left side of the cranium and the right side of the body. The opinion that the part of the system to the left side contains the collection of nerve-centres which act on the right side of the body, and that the centres on the right side act on the left side of the body, received in appearance a distinct anatomical sanction at the time when Mistichelli discovered the decussation of the anterior pyramids of the medulla oblongata, but things have changed since that time. That this decussation certainly exists cannot be doubted, but that the fibres of the anterior pyramids are really the means of communication between the brain, as the organ of the will, and the muscles, is a point in flagrant opposition to facts. It is even a question whether these fibres have any share at all in the conveyance of orders of the will to the muscles. I will possibly revert to this point again. I need not dwell at length on what is admitted as regards the physiology of the brain, as it is perfectly well known. But as regards certain points—namely, those which have become known since the discoveries made by Fritsch and Hitzig—I must say a few words. Since those discoveries a tendency to find centres serving to this or that function in the brain has been the fashion among physiologists and physicians. The facts that pathologists have furnished to the localisers have been taken hold of with a kind of passion, and shown to the whole profession and to the world as demonstrative proofs of the correctness of the views relating to those pretended centres of sensation, of motion, of vision, and so on. I have to show, in this course of lectures, that those facts, although many of them are perfectly correct, have not the very least weight in the matter, because we can find an immense number of facts which would prove exactly the reverse of what is apparently proved by the series of cases favourable to the views of the localisers. Or the other hand, I can show that those very facts, which seem to be favourable, can be explained very easily in another way, which has

this necessary superiority—that it is not only in harmony with those facts, but with all other facts, including those hostile to the views of the localisers.

It is essential, in order to understand fully what I shall have to say in these lectures, to recognise, in the first place, the immense power that any irritation possesses in producing a variety of phenomena of the most intense and violent description, and in the next place to recognise the difference between the cessation of a function and the loss of a function, as produced by such irritation. If you consider, for instance, what takes place when we take away a small bit of gray matter from the nib of the calamus in the medulla oblongata, you will have a clear illustration of the difference between the effect of irritation and the cessation of a function.

When M. Flourens made the experiment of taking away a small bit of gray matter from the nib of the calamus, he found that death was immediate. Had he pursued his study further he would have found that that death is accompanied by phenomena which are of a most interesting nature. But let us not now mind these phenomena. What is important for us is the immediate cessation of almost all the actions of life which then takes place.

Now if, instead of taking away the small bit of gray matter, we merely prick the neighbouring parts, we obtain, in most instances, all the phenomena that follow upon the extirpation of the bit of gray matter. If we concluded when we had taken it away that we had taken away the organ of certain functions, including those of circulation and respiration, there would be something perfectly true in the phrase of Flourens, that in taking away that organ we had taken away the vital force. But how is it when we prick instead of cutting a part away? The mode of death then cannot be the loss of function due to the carrying off an organ, for we leave it. The cause of the cessation of all the great functions is due to an irritation spreading to all the great nerve-centres, and producing an arrest of their activity, so that we see the cessation of interchange between tissue and blood, and the cessation of all the organic functions, excepting sometimes the action of the heart which, although weakened, may continue for a while. A prick of the medulla oblongata can then produce exactly the same symptoms as we observe after the extirpation of that part.

But it might be said that the functions annulled had their centre of activity in the part, however small, which has been pricked.

Fibres, no doubt, are destroyed which might be supposed to be endowed with such functions. It seems enough against that view that we have the whole of the effect, although we destroy only a very minute extent of fibre, and that we have that effect whether the prick is to the left or to the right, whether it is a little above or a little below the nib of the calamus. But not contenting myself with those facts, I will say that we can, by taking certain precautions, which I have no time to mention now, remove the whole of the pretended centre of vital force, the whole of the gray matter of the nib of the calamus, with some of the white substance round it, without having any of the effects that usually follow a prick or the extirpation of that mass of gray matter. We have, in that case, an illustration of the kind of mistake which is constantly made now, unfortunately, by physiologists and physicians, when they attribute functions to certain parts of the brain. In the case of the medulla oblongata, if that part were endowed with the functions attributed to it, or with the functions that are usually lost when that part is taken away, the effects obtained in some instances would always ensue from a complete extirpation of that part, as we cannot extirpate a part exclusively endowed with a function without producing a complete cessation of that function.

Another result of the experiments I have mentioned is, that the mere irritation of a few fibres can produce the most powerful effects on the heart, on respiration, on the mental and volitional activities, and on the interchange between tissue and blood—that is, nutrition. An excessive variety of effects, and also the most powerful effects, can, therefore, proceed from an apparently trivial irritation. We must all the time keep present to our minds this power of irritation when we study symptoms of brain-disease, as I have tried to show in my lectures on paralysis, in London, and as I shall try to show as regards other symptoms in my lectures here and elsewhere.

Symptoms of loss of activity—such as paralysis, anæsthesia, aphasia, and amaurosis—depend so little upon the loss of function of the parts where we find disease in the brain, and are so evidently secondary effects, due to irritation of the diseased parts, or their neighbourhood, that they may appear or not, and be the same or not, when there is disease of the brain anywhere you like—whether occupying one only or both the anterior lobes, or the middle lobes, or the posterior lobes, or the corpora striata, or the optic thalami, or the crura cerebri. In other words, let the lesion occupy either

one or two symmetrical parts of the brain, that lesion will produce symptoms, not because it has destroyed an organ possessing certain special functions, but on account of the irritation of parts that are not destroyed by the lesion, but surround those that have been destroyed—an irritation which, starting from that place, goes to other parts and stops their action, just in the same way as the galvanisation of the par vagum stops the activity of the nerve-cells which move the heart. What I shall endeavour to show—and this will be chiefly the work of my second and third lectures—is, that symptoms of brain-disease, consisting in a loss of function, appear, not because a diseased organ has lost its special function, but because an irritation starts from the place where the disease is, goes to a distance, and sometimes a very great distance, through even the spinal cord, and arrests the action of cells endowed with that special function. All that we know at present as regards the localisation of function—and I shall have to dwell upon this more at length by and by—is that the functions are not localised in clusters of cells existing in a zone in certain limited portions of the brain, but belong, as I have elsewhere tried to show, to cells which are scattered in the whole brain and other parts of the nervous centres. The cells, for instance, which serve for the faculty of expression of ideas by speech, and which, when stopped in their activity, produce aphasia, are scattered in the brain in such a way that disease can exist in any part of the brain without destroying that function.

I shall establish the two following series of proofs—first, that the parts which have been considered as the centres of sensation of vision and of the faculty of expression of ideas by speech, can be destroyed without either anæsthesia, amaurosis, or aphasia occurring; and, secondly, that a lesion anywhere in the brain can produce all those symptoms, and indeed that a lesion anywhere in the system, in the bowels, the bladder, or the kidneys, can also produce those very same symptoms, and evidently by the same mechanism. So that any one of the parts which are considered as endowed with these special functions can be destroyed without the function being lost; while, on the other hand, irritation, acting at a distance in any part of the nervous system, can produce an arrest of the activity of those cells which are really endowed with those functions.

After this general view of the subject, I must, in order to have it more completely accepted, insist on a variety of symptoms that

irritation can produce.   I spoke a moment ago of a prick of the medulla oblongata or of the corpus restiforme.   If we prick these parts we may see indeed a variety of symptoms showing how powerful an irritation may be produced, leaving aside the great symptoms that I spoke of a moment ago as resulting sometimes from such a prick.   There are other facts.   We may observe amaurosis to be produced at once in the eye on the side of the injury of the corpus restiforme.   We may observe hæmatoma of the ear, which not rarely occurs in certain forms of disease of the brain, producing insanity. We may find gangrene of the ear coming from the same cause; and we find also most various alterations in the lungs, and in the different viscera of the chest and abdomen, and great changes in the secretion of the urine, of bile, &c.—in fact, a great variety of effects showing how considerable and how various may be the results of the mere irritation of a few fibres.

· As regards other effects of irritation, if we take a bird—and this instance, indeed, is exceedingly striking—and pass a needle through the spine at the level of what has been called the spinal ventricle, that needle merely entering so as to touch the membranes that cover the fluid which is over that enlargement of the cord, the bird is immediately seized with locomotor ataxy; and if it were necessary to show that locomotor ataxy, when it results in man from disease of the posterior columns of the spinal cord, is merely the result of irritation, and not the effect of the loss of a pretended function of those columns, that fact in birds would certainly go to prove that locomotor ataxy may come from mere irritation.   If we prick any mammal—a dog, cat, or any other superior animal—in the posterior column of the spinal cord on one side, we sometimes see that all the effects, which were shown by myself, in this city in 1859, to result from the section of that half of the cord, are produced.   A pricking of a few fibres will give loss of the power of voluntary movement in the lower limbs on the same side, and it will give paralysis of the vaso-motor nerves on that side, and, in consequence of that, an increase of temperature.   It produces also hyperæsthesia, with paralysis of the blood-vessels, and an increase of temperature in the same limb, while in the other limb it produces anæsthesia.   This is certainly a striking experiment, proving conclusively, as I cannot but think, that the mere irritation of a few fibres possesses immense power in producing anæsthesia, hyperæsthesia, paralysis, changes of temperature, and so on.

I will not prolong this examination of facts to show the variety

of powers possessed by irritation.   One thing I must say, however, and that is, that if we consider the anterior pyramids in their proper light we shall find that, instead of being conductors of voluntary movement, as is supposed, they are links establishing communication between the brain and the body, for the purpose of acting on the nutrition of the body; and that being their function, when in the normal state, we unfortunately pay the penalty of the existence of that power when disease exists.   These front parts of the medulla oblongata are the means of great mischief in diseases of the brain, as they are probably the principal, if not the only channels of morbid action on the cells of the spinal cord, stopping their activity, and thereby generating paralysis in cases of brain-disease.   They are also the agents through which alteration in nutrition takes place; and, in fact, they certainly serve to produce the greatest morbid effects when there is disease of the brain.   The secondary degeneration of the spinal cord, which, as you well know, was first discovered by Ludwig Türck, and afterwards well studied in Paris by my friends, Drs. Charcot and Bouchard, takes place altogether through the influence of those fibres of the anterior pyramids.   Those fibres become degenerated, but, as Professor Vulpian has well shown, it is not always by a transmission of the disease gradually taking place by continuity of alteration along the same fibres, but by an influence from above acting only on certain parts of the length of the diseased fibres.   It is an influence exerted by those fibres on the blood-vessels in their neighbourhood, by which an alteration of nutrition takes place, there not being sometimes any continuous line of disease.

Professor Vulpian has not dwelt at length on this, but has given facts which, together with a great many others, lead to the view I have just mentioned.   It is not so always.   In most cases, on the contrary, the disease seems to extend along the continuity of the fibres.   But sometimes there is what has been seen by Mr. Curling and other surgeons in cases where tetanus has come from a traumatic injury, and in which it has been found that the nerve, starting from the wounded part, is diseased in many parts of its length, but with healthy intervals between the diseased ones.   Generally the nerve is not inflamed in all its length, but there are a number of cases in which it has been found to be so.

As regards the anterior pyramids there are some changes which, if we had time to dwell upon them, are of great interest indeed.   It is perfectly well known that the secondary degeneration which

takes place in a nerve after it has been divided is a continuous degeneration.   There has been a tendency among pathologists to consider the secondary degeneration of the anterior pyramids, and that in the lateral columns of the cord, as being due to something of the same kind—the disease of a pretended trophic centre in the brain being then considered as the cause of the degeneration. But that is not so.   The anterior pyramids, as Deiters has shown, throw their fibres into cells; and it is perfectly well known that cells arrest the propagation of that kind of degeneration which we find in fibres.

ANÆSTHESIA AS AN EFFECT OF BRAIN-DISEASE.

I will now pass to the subject of the first lecture.   Anæsthesia, as you well know, is a loss of any or of all kinds of feeling. I will not dwell on any point except my great object of showing that the views I had admitted as to the channels of transmission of sensitive impressions through the spinal cord to the brain must be modified.   First, I shall say that the view which I had myself so long accepted, and which admits that the lateral half of the brain on the left is the centre for the sensitive impressions that come from the right side of the body, is quite wrong.   I cannot but admit that each of the two sides of the brain has the power of receiving and appreciating the various qualities of the sensitive impressions that come from either side of the body.

There is one physiological experiment which is very much in favour of the view I maintain.   I have no intention of dwelling at length on physiological facts, as there are so many pathological facts of importance, but I cannot avoid mentioning this one.   If we apply the two points of a pair of compasses to the forehead, so that one point touches on one side of the middle line, and the other about half an inch above on the other side of the middle line, we shall find that only one point is felt. It is very rarely that the person on whom the experiment is made knows on which side the feeling exists, but one point, therefore, being felt, it would be natural that the sensation should be considered as belonging to the one side or the other, but it is very rare that there is any notion in that respect.   Still, when it exists, it is almost invariably the point which is the highest that gives the sensation of the side which seems to be the one which predominates in its action.   But if we had two brains distinct one from the other, and if the right brain were the centre

for receiving impressions coming from the left side of the body, and the left brain the centre for impressions coming from the right side, we should have, in an experiment of that kind, always two perceptions—we should feel the two points. It is not so, for I have tried it, and found it to be as I state for every part of the body. Indeed, if you try to what distance the two points can go when you separate them gradually one from the other, so as to reach a perception of both, you will find that there is no very great distance between the middle line and the parts of the skin at some distance from it. I think that this is almost a decisive experiment against the view that there are two distinct centres, one for impressions coming from the left side, and the other for impressions coming from the right side of the body.

But I will now pass to the pathological facts, as they will be found to be more decisive, and there are plenty of them. The facts which are the most decisive against the views that I fight against are those where the loss of sensibility has taken place on the side of the injury to the brain. There are many such cases, but not so many by far as there are cases of paralysis of motion taking place on the side of the injury to the brain, and that for the very simple reason that the proportion of cases of anæsthesia to cases of paralysis is extremely small. Although I really believe that there are always some traces of anæsthesia in cases of disease of the brain, it has, however, not been found or noticed in a great many cases; and it is very rarely found to be considerable. It is not so with paralysis, of course. Still, although the number of cases is not great, there are enough of them to show this great point—that there is hardly a part of the brain of any importance which has not been the cause sometimes of loss of sensibility—i.e., anæsthesia on the same side on which the disease existed; and on that point I must—as I love truth the most—at once mention two facts, one of which particularly is quite decisive against the physiological conclusion (from experiments) for which I fought so many years, and about which at present I cannot but consider that I was mistaken. It is not necessary to repeat the details of the experiments which I made in many places, and in this city in 1859, and which seemed to establish the truth of the conclusion alluded to.

You know that I tried to show by two sets of experiments—one consisting in a transverse section of a lateral half of the spinal cord, the other in a longitudinal section of a great part of that nervous

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no matter how you explain it. As Dieffenbach, I think, was the
first to show, and as Magendie afterwards established by many
facts which a great many among you must have had an opportunity
of seeing, in cholera the blood becomes such in the brain that there
is hardly any circulation there, and if there were, there would not
be that so-called *pabulum vitæ*—*i.e.*, oxygen in the blood (excuse
the word " so-called," which, though seemingly audacious, I do not
use as conveying an absolute denial of what is admitted), because
the blood in that case has not got enough of its pretended essential
vitalising agent. It has very little oxygen indeed, and certainly
much less than the amount which remains in the brain for more
than five or six minutes after ligatures have been applied to its four
main arteries. The blood is sometimes in a thick syrupy state, almost
in a coagulated condition—it is a kind of soft paste rather than
a liquid, and its circulation has been certainly very much impeded
if not stopped altogether in some parts. If in such a condition the
brain can remain active it is clear that one of two things must be
admitted—either there is in the poison of cholera itself something
that gives the brain that power of standing the lack of oxygen and
of proper circulation, or we must admit that the activity of the brain
can be maintained for a very long time without circulation and
nutrition, and in the absence of oxygen.

But in this last case we must admit also what a great many
facts render probable—that in those experiments which seem to
show that the cessation of circulation is a cause of cessation of
activity of the brain, there is simultaneously with the cause of
stoppage of circulation something else which causes a cessation
of activity in the cells of the brain, in the same way as gal-
vanisation of the par vagum arrests the activity of the cardiac
nerve-cells. Indeed a good many facts show that a mere prick
of the corpus restiforme sometimes stops the activity of the
brain immediately, leaving no sign of power of will or of mental
activity—and that sometimes when the heart is not stopped and
when breathing is not completely stopped; so that in that case
you cannot admit that the cessation of the activity of the brain
depends on the cessation of circulation. Besides, the activity of
the cells of the brain can be stopped even when there is no possi-
bility of interference with the amount of blood in the cerebral lobes,
as is the case when the sympathetic nerves have been divided in
the two sides of the neck, and when a prick of the medulla oblon-
gata destroys the cerebral activity. There are a number of facts—I

only mention this one—which clearly proves that we are not to look on a cessation of the circulation as the essential cause of the arrest of the activity of the cells of the brain.

If I had to dwell on those cases, and on the subject of the production of loss of consciousness in apoplexy, you would have a very easy explanation of what takes place. You well know that in cases of apoplexy the loss of consciousness may come even when the injury is extremely slight. You well know, also, that there is sometimes, without any hæmorrhage in the brain, from the mere irritation caused by inflammation, limited to the small part of the organ, a complete loss of consciousness—that is, of activity of the brain-cells—and therefore something similar to that which takes place when I prick the corpus restiforme. I have mentioned these facts, relating to the brain, to show that if we can find a great probability that the nerve-cells, used in voluntary motion or in mental actions, can have their activity arrested by an irritation starting from distant parts, we can also admit that a loss of feeling may be due also to the arrest of activity in nerve-cells from an irritation proceeding from more or less distant parts. This is the only rational explanation to be given of many facts relating to anæsthesia. For instance, if we see that a considerable alteration of the medulla oblongata can cause anæsthesia on the same side as that of the disease, we have a clear proof that an explanation like that just given can answer for such a case, while we must put aside altogether the view that the conveyance of sensitive impressions so takes place in the spinal cord that the right side of the body has its conductors on the left side of that nervous centre, and the left side of the body its conductors in the right side of that organ. Take a case like that of Dompeling, which is given in an American journal of mine, and in which a large tumour had destroyed almost the entire left half of the medulla oblongata, except the superior part of the anterior pyramid on that side, and in which the fibres belonging to the two anterior pyramids at their place of demarcation—namely, those on the side of the tumour before their crossing, and those of the opposite side after their crossing, were destroyed. In that case there was paralysis of motion and of sensibility on the side of the injury, the left. According to the admitted theories there should have been paralysis of movement in the two sides of the body, and loss of sensibility on the right side. This is a kind of *experimentum crucis*, made by nature, showing that the admitted views about the

pyramids are completely wrong, and that the views I held about the conductors of sensitive impressions are to be replaced by a more comprehensive theory.

In a case communicated by an able physician of Liverpool, Dr. Waters, to the Medico-Chirurgical Society of London, the patient had loss of motion, and also an incomplete loss of sensibility on the side, of a deep transversal section of the medulla oblongata, at the level of the corpus restiforme. In another remarkable case, mentioned by Mr. Stanley, a very able surgeon and good observer, there was complete replacement of the tissue of one lateral half of the pons Varolii by morbid tissues; and, in that case also, the loss of sensibility and of motion were on the side of the injury. There are other cases which are less decisive, perhaps, than these, but there is no question that injury in any part of the medulla oblongata, whether at the back or the front, or injury of the pons Varolii, or of the cerebellum, or of the crus cerebelli, by disease or from traumatic causes—injury also in any part of the base of the brain where the pretended centres of motion and sensibility are considered to exist—can produce paralysis of motion and sensibility on the same side.

Although I refrain from criticism generally, not liking to indulge in it, especially when I have to deal with a man of merit and a co-worker in physiology, I cannot but think that a most wonderful argument has been brought against the conclusions I had drawn from facts, showing that paralysis can come from disease on the same side of the brain. I will name the author I have in view, as I have no doubt he can bear to be blamed as well as I can, and as I certainly consider him a man of great merit—Dr. David Ferrier. In his wonderful book, published some time ago, on the physiology of the brain, among others things which have excited considerable surprise to my mind (perhaps not so much, however, as what I have published has excited in him), I found that he considers those cases I have collected of paralysis on the side of an injury as being something similar to what takes place when the heart is displaced in a man. The heart, as you know, is chiefly on the left side of the chest, and the cases of transposition in which it is on the right side of the chest, he considers as being anatomical anomalies similar to these he supposes to have existed in those cases in which paralysis has existed on the side of a brain lesion. If you read the demonstrations I have given, you will find that I have brought forward a good many kinds of facts,

for which the explanation of Dr. Ferrier could not hold good. I will only name a few. The first series consists in this, that paralysis on the side of the injury is by far more frequent in cases of disease in the part that I have called the *special corner* of the brain (because it is a puzzle to all pathologists)—I mean that part where the fifth pair of nerves enters the pons Varolii, where also the auditory and the facial nerves enter the medulla oblongata, and especially the angle formed by the crus cerebelli and the pons Varolii, including the neighbouring part of the medulla oblongata, than in any others. Even a superficial injury there more frequently produces paralysis on the same side as the injury than one in any other part of the brain. If we were to admit what Dr. Ferrier says, we would have great difficulty about this. How is it that in those individuals who, according to him, would have some malformation of their nervous system, the disease should come there chiefly? The same thing might be said of the posterior lobes of the brain and of the cerebellum, which also have more power of producing paralysis on the same side as the injury than other parts of the brain.

But there is a still better argument against the views of Dr. Ferrier, which is this:—There are cases—and I have collected a number of them—in which from disease occurring first in one part of the brain and then in the other, or from two consecutive injuries, the patient has had in the two instances hemiplegia in the same side of the body—in one instance, therefore, a direct paralysis due to one of the two lesions, in the other instance a crossed paralysis due to the other lesion. In those cases no malformation of the conductors of voluntary action could account for what took place. It is, besides, impossible to admit the view in question in presence of facts showing that a lesion in one half of the brain has produced simultaneously a paralysis in the same side, and another in the opposite side—a paraplegia or a paralysis of the two arms or that of the four limbs. What, also, of those cases in which two lesions having occurred in only one side of the brain, hemiplegia has shown itself in one instance on the same side, and in the other on the other side, so that there was once a direct, another time a crossed paralysis?

There are a good many other facts which show clearly that we are to consider the paralysis in those cases as depending on the effect of irritation which may act sometimes in one way and sometimes in another, and not on the destruction of conductors. And as

regards this variety of effects, let me say this—that we all know that irritation anywhere has a peculiar tendency to produce a variety of effects. And so it is if we admit anæsthesia, amaurosis, and aphasia, as well as paralysis, to be dependent on the effect of irritation acting at a distance. Sometimes these effects will occur from injury in one part and sometimes from injury in another, and that without any rule that we can find. Continuing what relates to the cases of direct anæsthesia, I will say that there are striking cases indeed, showing how little we are to reckon on what are supposed to be the functions of certain parts of the brain. There is a case mentioned by Abercrombie, who was certainly a good observer, of disease of both corpora striata, which, instead of producing a double hemiplegia—as, according to received opinion, should have been the case—produced anæsthesia only, and in one side of the body. One of the corpora striata was far more injured than the other, and anæsthesia appeared on the very side of the greatest alteration, but I repeat there was no paralysis. My friend, Dr. Auguste Ollivier, of Paris, gave me a most interesting case, yet unpublished, and the details of which I have here. It is a case in which disease of one of the optic thalami produced nothing but loss of feeling all over the face, limbs, and trunk, on the same side of the body. Another case, which I have not published yet, was given to me by a young surgeon, who was at the time in one of the London hospitals, Mr. Woodhouse. There was considerable disease of one half of the cerebellum, producing nothing but loss of sensibility on the corresponding side of the body. I myself saw, in 1850 or 1851, in conjunction with my friend, Dr. Tailhé, and published at the time, a case of tumour in one side of the cerebellum producing not only paralysis but also anæsthesia in the limbs on the same side. The cerebellum is certainly one of the organs in the brain which seems to have the greatest tendency to produce anæsthesia on the same side, as well as paralysis on the same side.

From the facts I have mentioned, and from many others, it is quite clear that a lesion occupying a part of the brain, the supposed seat of sensibility—as the optic thalamus or the medulla oblongata, or supposed to be out of the tract of sensitive conductors—as the cerebellum, can produce anæsthesia in limbs on the same side, which is a decided proof that our views about the place of passage of conductors of sensitive impressions, and the location of the centres of perception, are to be rejected.

I now pass to another argument. Anæsthesia from brain-

disease, even when the lesion occupies a supposed purely motor centre, may appear without any paralysis at all, and then it may show itself sometimes on the same side, but generally it will appear on the side of the body opposite to that of the lesion. If you take any of the parts of the brain which are considered either as motor centres or as conductors used in voluntary motion, there are a number of cases of disease of those pretended centres or conductors without any trace of paralysis of motion, but with loss of sensibility existing almost always, as I have said, on the opposite side, so that in these cases we have also something special against the views that are admitted about sensibility, and also about voluntary movements.

The third series of facts I shall mention goes to show that a disease occupying a part of the nervous centres, which has never yet been supposed to be at all endowed with any function, as regards sensibility—a disease localised in those parts and away from the pretended centres of sensibility, can, however, produce complete anæsthesia. There are many such cases, in which a disease striking cerebral convolutions—far from where some persons have the singular idea of localising the centre of sensation— or striking the cerebellum, as it was in two of the cases I have mentioned, or those parts of the pons Varolii, or of the crura cerebri, considered as motor and not sensitive—is capable of producing loss of feeling, generally, I must say, with paralysis, but sometimes without it.

If we take cases which seem to be favourable to the views generally held, such as those in which disease exists, either in the pretended centres of perception or along the pretended tract of conductors of sensitive impressions, we shall even there find many arguments against those views. In the first place, we shall find that where disease destroys the whole of the optic thalamus on one side—the optic thalamus being by many able physiologists considered as the centre of perception—the result may be anæsthesia in only one limb; and in one case it will be an upper limb and in another a lower one. You cannot attribute this to freaks of nature. If the optic thalamus be the centre of perception and of sensitive impressions, as has been supposed, the destruction of that centre ought always to produce the same result. But we find three things against that view. First, as I have said, anæsthesia may appear only in one limb instead of both, and it may be either an arm or a leg. Again, anæsthesia may not appear at all; and, thirdly, it

· may appear on the same side of the body as the disease instead of the opposite side, as should always be the case if the view in question were true. So that there are, at least, three sets of facts against that view.

What I have said about the centre I have just named, I might say with equal truth as regards any of the other pretended centres of sensibility. The optic thalamus is one of the most interesting organs in the system. But as I have, unconsciously, passed the limit of time allotted for this lecture, I must reserve what I have to say in reference to that organ to the next.

ART. II.—*On the Treatment of Amblyopia.*[*] By H. R. SWANZY, A.M., M.B., Dub.; F.R.C.S.I. ; Surgeon to the National. Eye and Ear Infirmary; Ophthalmic Surgeon to Steevens' Hospital; and Lecturer on Ophthalmic Surgery, Steevens' Hospital Medical College.

· THE following are a few cases of amblyopia which have been treated by a new method—namely, by Inhalations of Nitrite of Amyl:—

CASE I.—W. G., a coastguard, aged thirty-nine. On the 12th May last and following fifteen days he was employed in white-washing the coastguard premises. The sun shone brightly on the wall he was working at, and before the first day was half over he perceived the sight of each eye to have become dim, and he complained of pain in the temples. Still he continued the whitewashing for a fortnight. During this time his sight did not get worse; on the contrary the left eye recovered good vision, and he thought the right would also get well, but not finding it do so he applied to his medical officer on the 5th of June. By him he was placed on a mild course of mercury. A fortnight after this the sight of the left eye became dull again, and continued to get worse. The sight of the right eye had remained the same from the commencement. The patient was sent to the National Eye and Ear Infirmary under my care, on the 12th of July.

Upon examination, I found that with the right eye he could count fingers when held up at a distance of four feet only. The ophthalmoscope showed the vitreous humour of this eye to be

Oc Sin

Fig 1
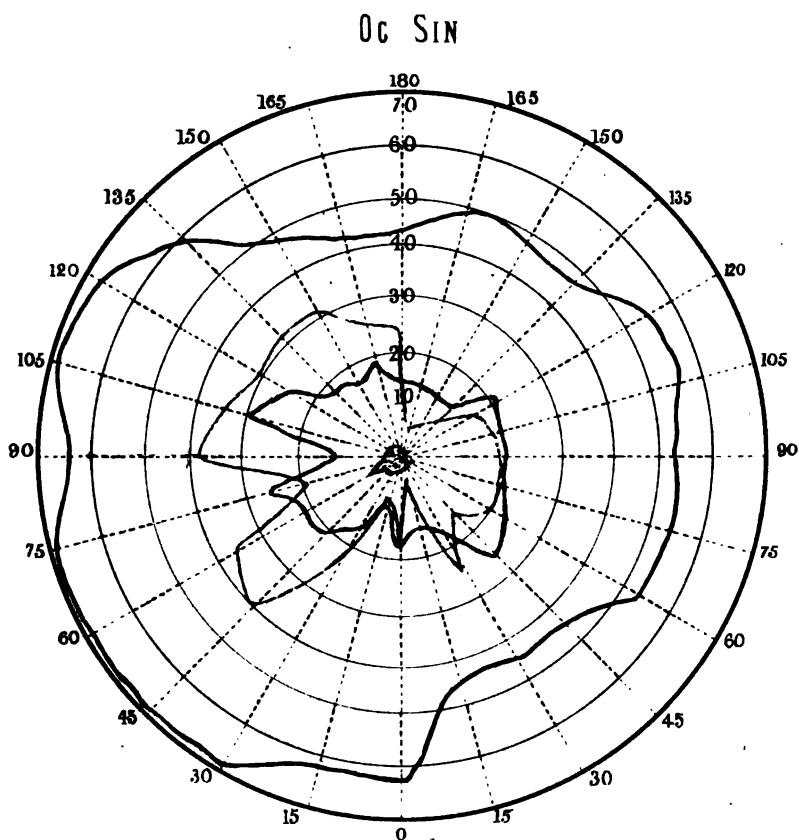
PERIMETER CHART,—CASE 1.

filled with a diffuse opacity, rendering any view of the fundus oculi impossible. The vision of the left eye amounted to something between $\frac{1}{3}$ and $\frac{1}{4}$ (L. Sn. at 14') of the normal standard. It could make out type No. $4\frac{1}{2}$ of Snellen at 8 inches with difficulty. He complained a good deal of a broken irregular appearance of the letters, and of temporal headache. The ophthalmoscopic appearances of the left eye were perfectly normal.

My prognosis for the right eye was unfavourable, as opacities of the vitreous humour of this kind are but rarely amenable to treatment, and there was no doubt but that the greater part of the defect in vision of this eye was due to this opacity. For the left eye I held out hopes of being able to do something, basing my opinion on the healthy appearance of the optic nerve and internal coats.

I then examined the field of vision (Fig. 1) of this eye by aid of Mr. Brudenell Carter's perimeter, using as a visual object a morsel of paper three lines square. The outer boundary (black line, visual object white) was but little, if at all, contracted. The apparent contraction above and to the inside, and below and to the inside, is due to the patient's prominent brow and rather aquiline nose. The power of perception of colours, however, was confined to a very much smaller portion of the field than normally. In a healthy eye, if the perimeter be placed so that the fixation point* be removed 18 inches from the eye, the blue boundary would reach to a distance of from 40° to 60° from the fixation point in every direction, and the red boundary would lie somewhat inside this, while the boundary for green would be found between 30° and 40° from the fixation point. In this case the furthest point of the blue boundary was at 40°, while at one place it was contracted to about 5°. The red boundary was also greatly contracted; but the most marked disturbance was in the power of perception of green, which was reduced to a very small district around the fixation point. Even within the boundaries indicated, the functions of the retina seemed easily wearied. Thus, if the white visual object were held motionless in one spot for a few moments, it disappeared from view; the retina, in fact, became fatigued at the spot and ceased to act. The red or crimson object quickly lost its colour if held motionless within the red boundary, becoming of an indistinct dark shade. Blue became paler, approaching to white, when held motionless.

As has been my usual practice in almost all cases of amblyopia,

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* In making perimetrical examinations, I am in the habit of examining from, or towards, the macula lutea, and not the blind spot.

I commenced the treatment by an application of Heurteloup's artificial leech to the temples; however, not finding any benefit from it, I did not repeat it.

On the 17th of July I caused the patient to inhale 10 drops of the nitrite of amyl from a small bit of cotton wool. Soon after he had quite recovered from its effects, I made him inhale 10 drops more. Five minutes after the effect of the second inhalation had worn off, I tested his vision, and found that a remarkable improvement had taken place for so short a space of time. He could now spell XL of Snellen at 14 feet—that is to say, his vision had improved from $\frac{1}{8 \cdot 5}$ to $\frac{1}{2 \cdot 8}$ of the normal standard. He was also able to read No. $4\frac{1}{2}$ with greater ease, remarking that the words seemed less confused than previously.

Upon testing the patient's vision next day, I found that it had not relapsed, but that the improvement which had been effected continued. He then got 10 drops to inhale. The peculiar symptoms produced by the inhalation continued about two minutes, and vision was not improved while they lasted. As they were just wearing off vision was slightly dimmed, but this disappeared in three or four minutes; 10 drops more were then given, and vigorously inhaled. About half a minute after the effect began, the letters seemed double their real size, but still in mist and broken. Two hours later the vision for distance was found the same as after the inhalations on the first day, but near vision was improved so that he could read No. $3\frac{1}{2}$ of Snellen at 8 inches slowly.

On the 20th he was beginning to see XXX of Snellen at 14 feet. I gave him three inhalations; and about three-quarters of an hour afterwards he could spell XXX at 14 feet almost perfectly (OSL*O*T*O*). No. 3 was read slowly at 8 inches.

On the 21st vision was no better, nor did two inhalations on that day effect any further improvement.

On the 22nd vision still remained the same, but, after two inhalations, the sight became so improved that XXX could all be distinguished at 14 feet. He still complained of the haziness, although he said it was lighter. He said also he could only see the letter he looked at and the one next it, while the others were quite in mist.

On the 23rd he got two inhalations.

On the 24th vision was not any better, but after two inhalations he said he could make out XXX much more clearly. This vision now was very nearly half of the normal standard, and I did not

succeed in producing any further improvement in it. I continued the inhalations of nitrite of amyl for a time, and tried some hypodermic injections of the nitrite of strychnia, but without avail. Before dismissing him from hospital (8th August) I again examined the field of vision with the perimeter, and found an improvement in the boundaries for red and blue, but very little in that for green.

On the 20th September he returned, complaining that about eight days previously the sight of the left eye had again become dim. He thought he had exposed himself too much to the bright glare of the sun. Bright light always gives him uneasiness and pain in the temples. I found his vision reduced as low as on the first day I saw him. After two days treatment with the nitrite of amyl his sight was restored again as before. He was cautioned against exposure to strong lights in future.

CASE II.—James O'H., aged sixty, a nailer, was admitted to the National Eye and Ear Infirmary on the 19th May last. His sight had commenced to fail 18 months before, but he had been able to work at his trade until within four months, when, in the course of a week, the sight became greatly worse and continued so. For thirteen weeks prior to this rapid deterioration in his sight he had been drinking " weighty," as he himself expressed it, rarely taking his clothes off. During this time also he smoked an ounce of twist tobacco in the day. His habit for the last seven years had been to drink and smoke a great deal.

The ophthalmoscope displayed a loss of the natural pink hue of the optic discs, and in its place a dirty white discoloration. The central artery of the retina was not altered, and the central vein was also normal.

With each eye the patient could count fingers at six feet only, and ($+14$) could read but a few letters of No. 12 at eight inches. An examination of the fields of vision showed the outer boundary in each eye to be normal. The power of perception of red was not materially affected, but the powers of perception of blue and green were wholly wanting. In consequence of the questionable appearance of the optic nerves and the defect in the power of perception of colours, I did not hope to effect any great improvement in his vision; however, in this prognosis, I am happy to say I was mistaken.

On the supposition that in cases of this kind the disease consists in a defective nutrition of the nerve substance, I had determined to

try the effect of a nerve tonic, in the form of phosphorus, when the next opportunity might arise. Accordingly, after the artificial leech had been applied once to each temple without benefit, I placed the patient on Kirby's pil. phosphor., containing $\frac{1}{35}$ grain of phosphorus each, one three times a day, at meals. I need hardly say that all smoking and drinking was strictly forbidden, and I believe the patient attended to my directions in these respects.

The phosphorus treatment was commenced on 29th May. I need not weary the Society with a diary of the case; suffice it to say that on the 1st of July the vision of the right eye had improved almost to C of Snellen at 14′, and that of the left eye to C C at 14′, while with each eye (+14) some words of No. 5½ Snellen could be read at 8 inches. The field of vision also had improved, inasmuch as blue could now be well distinguished as far as its normal boundaries, but green could not yet be seen. On the whole the result of the phosphorus treatment in this case must be regarded as very satisfactory when the serious nature of the case is remembered. I consider the method well worthy of an extensive trial in similar cases. I cannot find that any late writer has recorded cases treated in this way, but it seems, from Dr. Ashburton Thompson's book* (p. 158), that about twenty years ago Löbel treated some cases of glaucoma (?) with phosphorus successfully, while Gosselin and Maisoneuve record some negative observations.

Having experienced the beneficial results of the nitrite of amyl treatment in the first case, I determined to try it in this one also, and, on the 24th July, I gave the patient 10 drops to inhale. Fifteen minutes later the vision of the right eye was improved so that he could almost see C of Snellen at 14 feet—that is to say, his vision in this eye was improved from about $\frac{1}{14}$ to nearly $\frac{1}{4}$ of the normal standard. The left eye had not improved. He then got 10 drops more, and an hour later the vision of the left eye had increased to Snellen C at 14 feet.

25th July.—V. as yesterday. Two inhalations. Half hour later no further improvement.

26th July.—*Oc. Sin.* No improvement. Reads No. 5½ words, with +14.

Oc. Dex. V.$=\frac{14}{C}$ (+14) No. 6½ (words) at 8″.

Two inhalations. Half hour later no further improvement.

27th July.—*Oc. Sin.* Seems to be beginning to see LXX at 14′. Can say that the first letter is D.

* Free Phosphorus in Medicine. 1874.

Oc. Dex. Unimproved since yesterday.

One inhalation. After half hour *Oc. Sin.* $\frac{14}{LXX(?)}$ (D?N). *Oc. Dex.* $\frac{14}{C}$ Second inhalation. Afterwards, *Oc. Sin.* $\frac{14}{LXX}$ *Oc. Dex.* $\frac{14}{LXX?}$ (DL).

30th July.—Two inhalations. V. unaltered.

31st July.—V. unaltered.

1st August.—*Oc. Sin.* Stat. id. *Oc. Dex.* $\frac{14}{LXX}$

2nd August.—*Oc. Utr.* $\frac{14}{LXX}$ rather clearer with *Oc. Sin.* than *Oc. Dex.*

3rd August.—*Oc. Utr.* No alteration. After three inhalations no further improvement.

No further improvement in V. was noted until the 12th August, the inhalations having been continued daily with the exception of two days.

12th August.—*Oc. Sin.* $\frac{14}{L?}$ (??BR). *Oc. Dex.* $\frac{14}{LXX}$ Three inhalations.

15th August.—*Oc. Utr.* $\frac{14}{L(?)}$ (??SR). Three inhalations.

17th August.—*Oc. Utr.* as on 15th inst. Three inhalations.

18th August.—*Oc. Utr.* V. Stat. id. Three inhalations.

19th August.—*Oc. Sin.* $\frac{14}{L(?)}$ (??SR). *Oc. Dex.* $\frac{14}{L(?)}$ (?TSR). One inhalation.

21st August.—*Oc. Utr.* as on 19th inst. *Oc. Utr.* (+14) No. 5½ (words).

Treatment continued daily without further improvement until

30th August.—*Oc. Sin.* $\frac{14}{L(?)}$ (?TSR). *Oc. Dex.* $\frac{14}{L}$ Three inhalations.

1st September.—*Oc. Utr.* $\frac{14}{L}$. With both eyes open V $=\frac{14}{XL}$ (+12) No. 5½ at 8″.

The treatment was continued for some days longer, but no further improvement was effected.

The improvement effected by the nitrite of amyl treatment in the course of six weeks was such, that the patient could at the end of this time see four times as well as at the beginning. Before dismissing him from hospital I again examined his field of vision, and found that the power of perception of green had returned and occupied a normal extent of the field.

CASE III.—Patrick C., aged thirty-eight, a shoemaker, applied at the National Eye and Ear Infirmary, on 17th July last, complaining of failing vision. He had been for many years a great smoker, and habitually drank very much.

With the right eye (Em.) he could see Snellen L at 14 feet with great difficulty. The vision of this eye was therefore about $\frac{1}{3}$ of the normal standard. He could read with the same eye (+20) No. $2\frac{1}{3}$ Sn. at 8″ with difficulty. He was not a good reader.

With the left eye (Hm. $\frac{1}{30}$) he could barely see Sn. C at 14 feet, an amount of vision equal to less than $\frac{1}{6}$ of the normal, and could read Sn. $5\frac{1}{2}$ (+20) at 8 inches.

The optic nerves seemed to be slightly whiter than normal. The corneæ presented signs of keratitides in childhood in the form of small facets.

An examination of the fields of vision did not discover any abnormality.

On 28th July he got 10 drops of nitrite of amyl to inhale. Ten minutes later the sight of the left eye had improved almost to Sn. L at 14 feet—that is to say, its vision was just twice as good as before the inhalation. The right eye had not improved. He then got 10 drops more to inhale. After thirty minutes the left eye could make some attempt at XL, while the right eye could see L more distinctly than before.

The vision of each eye went on improving by degrees under the treatment until the 8th August. It then had reached in the right eye almost to $\frac{1}{2}$ (XXX (?) at 14 feet), and in the left to more than $\frac{1}{3}$ (XL at 14 feet) of the normal amount. No further improvement could be effected. Probably perfect vision would have been restored had it not been for the irregularities on the corneæ.

The only disagreeable consequences which I saw result from the inhalations of amyl were in this case. While the immediate effect of the inhalation lasted, the Schneiderian mucous membrane secreted copiously, and there was an abundant flow of tears; and after a few days the inside of the nose became very sore.

CASE IV.—William P., aged forty, a messenger, applied at the National Eye and Ear Infirmary on the 24th July last. He said he had enjoyed very good sight until a week before, when it rapidly became obscure. He had always been in the habit of drinking three or four bottles of ale in the course of the day, and three or four glasses of whiskey in the evening, and of smoking half an ounce of twist tobacco every evening.

The vision of the right eye (E.) was about $\frac{1}{7\cdot1}$ $\left(\frac{14}{C\,(?)}\right)$ of the normal standard, and it could read (+20) No. $12\frac{1}{2}$ at 8 inches slowly.

Oc Sin

F.g 2.

PERIMETER Ch

Oc Dex
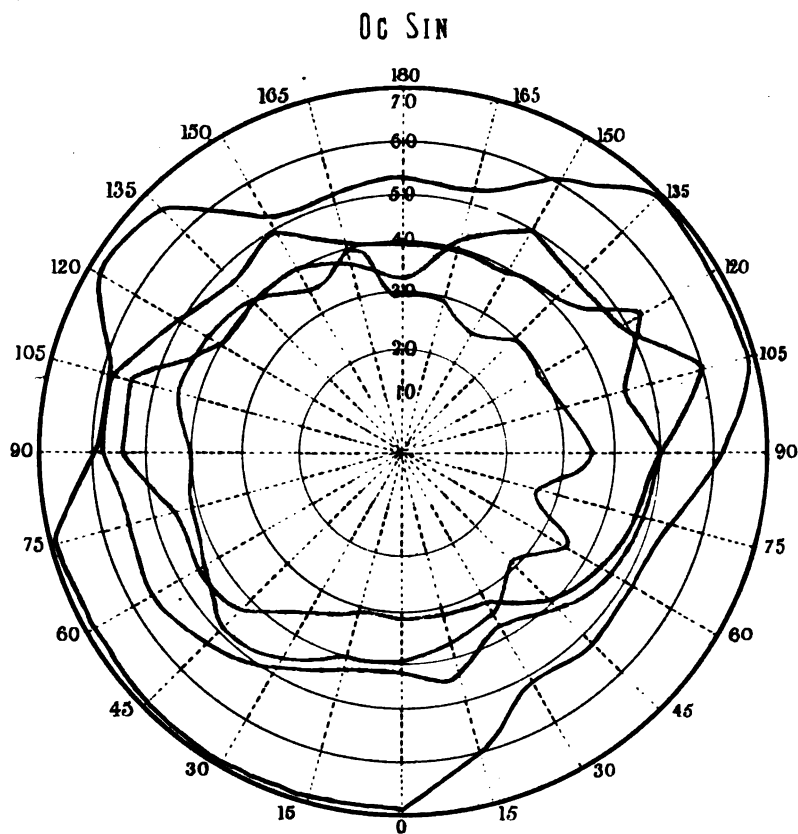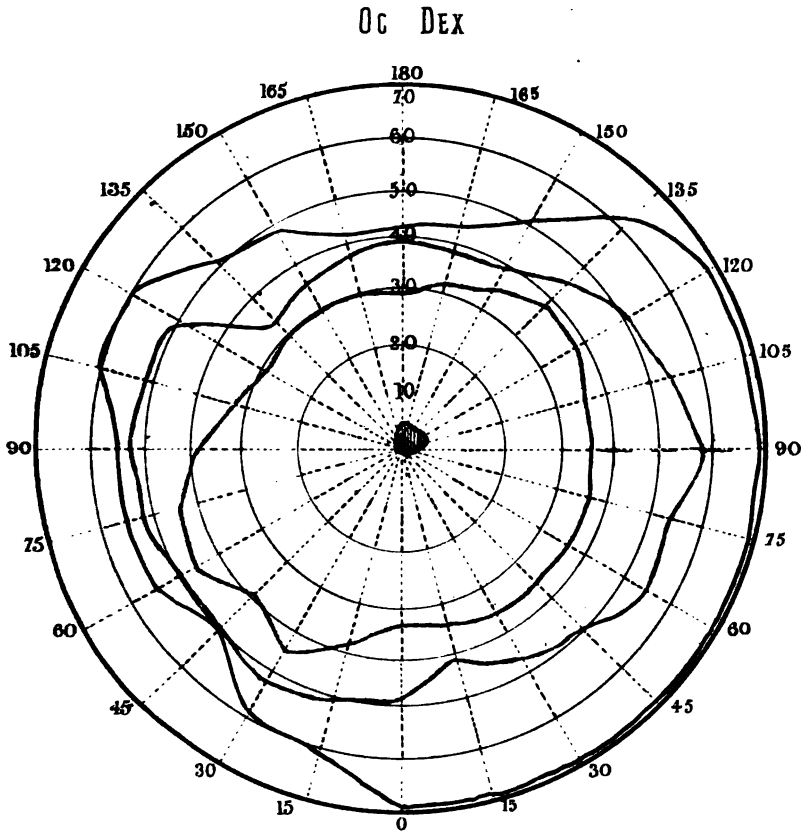
Fig 3.

RTS.—CASE IV.

The vision of the left eye (E.) was $\frac{1}{2 \cdot 8}$ $\left(\frac{14}{XL}\right)$ of the normal standard, and it could read $(+20)$ No. 3 with difficulty at 8 inches.

An examination of the fields of vision showed that in the right eye (Fig. 2) the outer boundary was normal, as also the blue boundary, while the field for red was somewhat contracted, and the power of perception of green was entirely wanting. In addition there was a central scotoma for red, a region just at the point of fixation, in which the power of perception of red was absent. When a bit of blue paper $2\frac{1}{4}$ lines square was held at the point of fixation it seemed to the patient brown in the centre, while around the edges it was blue; in short, a very small central scotoma for blue was also present. Yellow was seen as red in every part of the field except at the point of fixation, where it seemed brown.

In the field of vision of the left eye (Fig. 3) the outer boundary, as well as the red and blue boundaries, and the boundary for yellow, occupied normal positions, but, as in the right eye, green of every shade seemed to be brown in all parts of the field. There was no central scotoma in this eye.

The ophthalmoscope displayed no well-marked disease, but some slight paleness of the optic discs may have been present.

The patient was advised to abstain from all smoking and drinking, and on the 3rd August I gave him two inhalations of nitrite of amyl closely following each other. Upon testing his vision afterwards, I found that with the left eye he could see Snellen XXX almost perfectly $(GBLCTG)$ at 14 feet, being an improvement of from $\frac{1}{2 \cdot 8}$ to $\frac{1}{2 \cdot 1}$ $\left(\frac{14}{XXX}\right)$ in the course of a few minutes. The improvement in this eye continued advancing until the 19th August, when he could see with it XX Sn. almost perfectly at 14 feet $\left(V=\frac{1}{1 \cdot 4}\right)$. The right eye did not improve during the whole course of the treatment. He now remained absent until the 25th August, and in the meantime had, I suspect, returned to his old bad habits. On examining him on that day I found that some relapse had taken place in the left eye, its vision being but $\frac{1}{2 \cdot 1}$. He then attended for a few days and again remained away, so that no further improvement was produced by the treatment. I regret to say the patient did not give me an opportunity of examining his field of vision at the conclusion of the treatment. I saw him two months afterwards, and found that the vision of the left eye had fallen back somewhat, but I have no doubt that he had not maintained those temperate habits which I recommended to him as necessary for the preservation of his sight.

Besides the foregoing I have tried this method of treatment in one case of advanced atrophy of the optic nerve, and in two cases of neuro-retinitis, but without any beneficial result.

The only published case of amaurosis or amblyopia treated by the nitrite of amyl, of which I am aware, occurs in the number of the *Berliner Klinische Wochenschrift* for 24th April last. It is by Dr. Steinheim, of Bielefeld, and it suggested the trial of the drug to me in the above cases.

Dr. Steinheim's patient was a married woman, who completely lost the sight of both eyes after a severe hæmorrhage, connected probably with a miscarriage. The ophthalmoscope displayed a greyish white opacity of the optic papillæ, which were somewhat swollen and with indistinct margins. On the sixth or seventh day after the blindness came on, Dr. Steinheim admitted the patient into his hospital, and gave her 8 drops of the nitrite of amyl to inhale. As soon as the effects had passed off he gave her a second similar inhalation.

About fifteen minutes later sight began to return to the left eye, so that fingers could be counted at a distance of four or five feet. The right eye still remained blind. The patient was kept in a dark ward, and received good diet and wine. Nitrite of amyl inhalations were given several times a day. On the 30th of May the patient could count fingers at fourteen feet with the left eye, although only with the upper part of the field of vision. The lower part of the field did not recover its functions, nor did any part of the retina of the right eye. The appearance of the optic discs had altered to that of atrophy. Dr. Steinheim says he has had very satisfactory results from the treatment in other cases, which he proposes to make known at a later period.

I shall not venture to draw any definite conclusions from the cases I have narrated as to the precise indications for the employment of nitrite of amyl in amblyopia. We must wait for wider experience to establish these. It is evident, however, that there are some cases in which the treatment has a remarkable effect, and my object has been to draw attention to this fact, so that others may be induced to try the method.

As to the *modus operandi* of the treatment, I have been unable to do more than form a conjecture. The physiological effect of an inhalation of nitrite of amyl is to paralyse the vaso-motor nerves of the head and neck. I am inclined to think it is the increased supply of blood to the impoverished nerve-centres, caused by the

dilatation of the capillaries, that is the immediate cause of the improvement in vision.[a]

I may here remark, that in none of my cases did the ophthalmoscope show any alteration in the calibre of the retinal vessels while the primary effects of an inhalation lasted. This coincides with the observations of Steinheim, and of Sämisch and Stammeshaus,[b] but is at variance with those of Aldridge.[c]

None of the authorities whom I have been able to consult record any dangerous effects from the use of this drug. Wood, an American practitioner, says[d] the pulse should be carefully attended to, as there is danger of paralysis of the heart with too large doses. This danger, he says, never comes on suddenly, but gives warning long before by the nature of the pulse. He does not seem to have seen any fatal case. With such doses as I gave in the cases I have narrated I would not fear paralysis of the heart; but if I were suspicious of atheroma of the arterial coats, I would not give nitrite of amyl inhalations

The following list of authors who have directed attention to the uses of nitrite of amyl and its physiological action, although incomplete, may be of some interest:—

C. Höstermann.—"Ueber die Anwendung des Amyl-nitrits bei Melancholie."—*Wien. Med. Wochensch.* Nos. 46, 47, 48. 1872. *Centralblatt.* No. 10. 1873.

Found it most useful in melancholy madness, contributing materially to the permanent cure. 4–5 drops every fourth hour.

Robert Pick.—"Ueber das Amylnitrit und seine therapeutische Anwendung."—*Centralblatt.* No. 55. 1873.

Neither headache nor unconsciousness was produced even by the largest doses, the effect passing off rapidly without leaving any disagreeable results.

P. has proved experimentally that the nitrite of amyl is a direct muscle-poison. He considers it a most useful agent in hemicrania angio-spastica. It was most efficacious in a case of cardial neuralgia,

[a] Bernheim (Pflüger's Archiv, VIII., 253–259, 1873) and Eulenburg and Guttman (Reichart and Du Bois' Archiv, 1873, V., 620–625), find that nitrite of amyl paralyses the vaso-motor centres, while Pick's experiments tend to prove it to be a muscle-poison (Deutsch. Archiv. f. Klin. Med., XVII., p. 129).

[b] Centralblatt f. d. Med. Wissensch., No. 55, 1873.

[c] The Ophthalmoscope in Mental and Cerebral Diseases. West Riding Lunatic Asylum Reports. Vol. I., 1871.

[d] Amer. Journ. Med. Sc., 1871, p. 359.

and in several cases of epilepsy and asthma, and it had a temporary effect in a case of traumatic tetanus.

H. C. Wood, Jun.—" On the Therapeutic Value of Nitrite of Amyl."—*Amer. Journ. Med. Sci.* 1871. Pp. 359–362. *Central-blatt.* 1872. No. 14.

One case of tetanus successfully treated with the nitrite of amyl alone, and another case with similar result where it was used in combination with hydrate of chloral. Says its usefulness in angina pectoris, with or without organic disease of the heart, cannot be doubted. He describes a case where angina pectoris occurred in conjunction with mitral valve disease. The inhalation of five drops rapidly arrested each attack, and the attacks then appeared more rarely, until at last they ceased altogether. Thinks the pulse should be carefully attended to, as there is danger of paralysis of the heart with too large doses. This danger never comes on suddenly, but gives warning long before by the nature of the pulse. He does not seem to have seen any fatal case.

S. Weir Mitchell.—"Nitrite of Amyl in Epilepsy."—*Philadel. Med. Times.* 1872. No. 38. *Centralblatt.* 1872. No. 23.

Was able in two cases to prevent the occurrence of the attack by administering nitrite of amyl as soon as the premonitory symptoms appeared.

W. E. Jenks.—" Case of Puerperal Eclampsia, treated with the Nitrite of Amyl."—*Philadel. Med. Times.* 1872. No. 45.

Found it of great benefit in a case of severe puerperal eclampsia. The attacks were cut short, and soon ceased altogether.

J. Crichton Browne.—"Nitrite of Amyl in Epilepsy."—*West Rid. Lun. Asyl. Reps.* III. 1873. Pp. 53–174.

An inhalation either prevented the occurrence or diminished the severity of the fits.

Schüller.—" Ueber die Einwirkung einiger Arzneimittel auf die Gehirngefässe."—*Berl. Klin. Wochenschr.* 1874. Nos. 25 and 26.

S. observed a distinct dilatation of the arteries and veins of the pia mater. A dilatation of the vessels occurred even on the side upon which the vessels were already dilated, owing to an injury to the sympathetic, and it took place both after division of the pneumogastric and in curarised animals. The author thinks the

effect depends on the degree of filling of the vessels and upon the power of reaction of their middle coat.

A. Ladendorf.—" Ueber das Verhalten der Kopftemperatur bei Amylnitritinhalationen."—*Berl. Klin. Wochenschr.* 1874. No. 43.

The temperature in mouth and arm-pits rises considerably, and does not generally fall to its previous level for two hours.

M. Putnam Jacobi.—" Nitrite of Amyl and Belladonna in Dysmenorrhœa."—*Med. Record,* Jan. 2, 1875. *Centralblatt.* No. 34. 1875.

Two or three drops of nitrite of amyl to be inhaled, if a previous treatment with belladonna has not prevented the occurrence of dysmenorrhœa.

T. Jones.—" Suggestions for the Employment of Nitrite of Amyl in the Collapse and Cramp of Cholera."—*Brit. Med. Journ.* 1871.

On the supposition that the cholera-poison produces a contraction of the small arteries, and hence the stadium algidum and the muscular cramps, the author proposes to employ nitrite of amyl in this disease.

T. Lauder Brunton.—" Ueber die Wirkung des Salpetrigsauren Amyloxyd auf den Blutstrom."—*Arbeiten aus d. Physiol. Anstalt. zu Leipzig.* 1869. Pp. 101–120.

The blood-pressure in the vessels sinks, owing solely to the dilatation of the vessels.

O. Berger.—" Zur physiologischen und therapeutischen Wür-digung des Amylnitrits."—*Allg. Med. Centralzeitung.* 1874. No. 94.

B. shows by experiments on animals that the diminished blood-pressure may be produced by nitrite of amyl independently of the vaso-motor centres in the medulla oblongata. He says the drug is of use in the angio-spastic form of hemicrania only, and not in all cases. It may be effective in those fits of epilepsy with vaso-motor aura, but has no influence over the disease itself. Found of use in some cases of angina pectoris, in others not. It is of no use, according to the author, in attacks of bronchial asthma, in hysterical convulsions, or in tetanus. He twice found it have a beneficial effect in severe fainting fits.

C. Bader.—" The Dangers of Chloroform, &c., and the Nitrite of Amyl."—*The Lancet.* 1875. No. XIX.

B. recommends nitrite of amyl as an antidote where threatening signs make their appearance during a chlorform narcosis, and communicates three clinical observations in illustration.

S. Mayer and J. Friederich.—" Ueber einige physiologische Wirkungen des Amylnitrits."—*Archiv. für Exper. Pathol.* V. P. 55. *Centralblatt.* No. 38. 1876.

Experiments on animals:—The frequency of the pulse is increased in consequence of a depression in the tone of the pneumogastric centres. The drug acts directly on the walls of the vessels. After moderate doses the respiration becomes deeper and more frequent, but after large doses shallow and very slow.

Kelp.—" Amylnitrit."—*Deutsch. Archiv. f. Klin. Med.* XV. P. 602. *Centralblatt.* No. 10. 1876.

In five cases of melancholia stupida the author had negative results from the use of nitrite of amyl.

Clapham.—" Nitrite of Amyl in Sea-sickness."—*Lancet.* No. 8. 1875.

Out of 124 cases, 121 were completely relieved.

ART. III.—*Cases of Chorea in Relation to Treatment.*[*] By J. MAGEE FINNY, M.D. Univ. Dubl.; Fellow King and Queen's College of Physicians; Physician to the City of Dublin Hospital.

THE cases of chorea which I bring under the notice of the Society are, though entirely uncomplicated and simple, well-marked and typical. In briefly describing them I shall refrain from considering the wide subject of chorea in its pathological aspect, not only because in none of my cases was there a fatal termination, but also because the whole subject of brain-diseases and nervous affections is at present veiled in such obscurity, that for me to ask you to follow me through the diverging paths of opinion—the one leading to the localisation of chorea in certain portions or centres of the brain, and the other to the denial of any one central origin, or that there is any structural change at all to be found in the brain in chorea—would be on my part at once improper and presumptuous.

* Read before the Medical Society of the King and Queen's College of Physicians, Wednesday, December 6, 1876. [For the discussion on this paper, see page 95.]

Nay, in particular would it be injudicious for me to introduce such a subject while these walls are, as it were, still echoing the learned words of the great scientist, as he threw to the winds the doctrines and theories of nervous physiology and pathology which he had not only originated, but had nurtured up by arduous study for fifteen years.

My object is to bring under your notice the advantages of a certain line of treatment, without in the least claiming that it is either new and untried, or that it is the best and only treatment to be adopted in chorea.

It is a line of treatment, however, which, as far as I can judge from observation of the treatment of chorea by other practitioners and the perusal of the cases recorded in our journals, is one that has, of late, rather fallen into disuse and discredit.

CASE I.—*Severe Chorea; no history of Rheumatism; no cardiac complication; Treatment by Sulphate of Strychnia and Ether-spray, commenced a fortnight from the beginning of illness; Immediate benefit; Recovery in fifteen days. Total duration, four weeks and one day.*

Richard F., aged thirteen, came under my care, in the City of Dublin Hospital, 15th May, 1875, a fortnight ill. No cause for the attack could be obtained, except that he was a very studious boy, extremely fond of reading, and used to devote most of his out-of-school hours to study. He was a fair-haired, well-nourished lad, and presented none of the anæmic symptoms so common in choreic patients. Before he came into hospital he had bitten his tongue on several occasions, but, although the movements of his body and extremities became much more excited for some days after admission, the tongue escaped further injury.

This lad was a well-marked example of severe choreic disturbance—for, in addition to the ordinary "madness of the muscles" of the eye-lids, lips, forearms, hands, and legs, he was totally unable to stand or take a single step. His whole body, as he lay in bed, was so jerked and thrown about, that it was found necessary to prevent his falling out by tying a folded sheet across the bed; while any attempt to lift him up in the bed threatened dislocation of the head, so violent and jerking were his nods. His speech was also greatly impaired by the want of co-ordination of the expiratory muscles and those of the tongue and lips, a sucking inspiration continually interrupting even the shortest sentences.

The treatment he had received before I saw him had consisted of succus conii and shower-baths, with beef-tea and milk dietary.

On 15th May, I commenced the administration of strychnia in doses of $\frac{1}{60}$th grain of the sulphate three times a day.

On 17th the dose was increased to $\frac{1}{48}$th grain, and each day it was increased little by little.

The prescription I adopted is one recommended by Hammond,[*] which, by its accuracy and the facility of regulating the dose, supersedes that of Trousseau.

A solution of sulph. strychniæ, of half the strength of that of the British Pharmacopœia, is made, so that five minims shall represent $\frac{1}{48}$th grain of the alkaloid. Sufficient is ordered for one day, and syrup added. Each day an additional minim of this solution is added to the dose of the day before, so as gradually to reach the dose which will produce the physiological effects of this powerful excitant, or which will suffice to check the excited movements of the muscles. In the case before us I employed, in addition to the strychnia, what seems to me to be a useful adjunct—namely, the application of the ether spray to the spine, from the nape to the sacrum—a line of treatment, introduced by Zimberlin and Lubilski, which is said of itself to be curative.

On the 17th I first applied it to this boy's back for four minutes. The operation was not very easily carried out, owing to his excited state—increased, doubtless, by the alarm at the novel remedy, so that it became necessary to have him held during its application. Immediately the spray was stopped and his fear passed away; the patient said he felt better, and he could speak with more ease.

It is commonly the case that the first application is dreaded most, and gives rise to most alarm, but afterwards this fear gives way to one of actual pleasure; so it was in this boy, for on the second occasion (21st May) he did not seem to mind it, and on the third and last (the 26th) he said he liked it, and, to all appearances, he enjoyed its application. After each spraying he always expressed himself more clearly and intelligibly, and said he felt steadier and better.

On the 19th and 20th May—that is, in four or five days from the beginning of the treatment when the dose of the strychnia had reached only $\frac{1}{30}$ gr. ter die.—there was marked evidence of improvement. The patient had much more command over the muscles of

* Diseases of the Nervous System. 3rd ed., p. 616.

his hand, so that by an effort (of certainly no long duration) he could keep it steady, and open and shut each finger in succession. He no longer needed the restraint of the folded sheet across him; he could also stand and walk very fairly with help; but the nodding of his head still persisted, and seemed beyond his control, so that it gave him a curious appearance as he walked. The next day he was able to feed himself; he was allowed up on the 22nd—the seventh day of treatment; and on the 26th he was able to walk out in the grounds of the hospital unsupported, his head being now quite steady. He was also able to hold a book and read it, and, in fact, he spent most of the remaining few days of his stay in hospital in this occupation. He left hospital, seemingly quite well, on the 29th, having been under strychnia treatment fourteen days. The highest dose of strychnia reached in this case was $\frac{1}{17}$th grain on the 27th. It, however, produced none of its physiological effects, such as pains and stiffness in the neck, arching of the back, or cramps and pains in the legs. The only marked effect seemed to be the production of an enormous appetite for bread, of which the patient devoured, in addition to his allowance, an extra medium-sized loaf per diem.

Ten days after leaving hospital he came back with a slight return of the unsteady movements, but these passed away in a few days, and they were very probably induced by his eager haste for study, as I learned he had, immediately on leaving hospital, resumed his studies at a school in Marlborough-street.

The total duration of chorea in this case—excluding the relapse of a few days—was four weeks and a day. Improvement followed four days treatment, and cure was effected in fifteen days.

I am indebted for the notes of the foregoing case to Mr. W. Fraser, my clinical clerk.

CASE II.—*Severe Chorea; primary attack, bilateral; no Rheumatism or Cardiac Disease; Treatment by Strychnia and Ether-spray; Improvement in Four days; Cure in Twenty-five days of Treatment. Total duration, six weeks and three days.*

[Reported by Mr. Donelan, L.K.Q.C.P.I., practising pupil in 1875.]

Catherine T., aged eleven, was admitted into the City of Dublin Hospital on the 5th January, 1875. A week before admission I saw her in the extern department of the hospital, having walked thither. Her symptoms then consisted of grimaces, awkward jerky mode of walking, and dragging, unsteady motion of the left foot;

but between that day and the 5th, the choreic movements became much worse, and were so aggravated that she was unable to stand or walk, and had to be brought in a cab to the hospital and carried up stairs. She is the daughter of a man who had a year ago left the army, and seemed to lead a vagabond life. Her mother died two years before, and she has since been living a sort of gipsy life with her father, and by her general appearance, as well as by the presence of two spots of tinea circinata, it is evident the child was much neglected. Eight months before, while in Manchester, she had been ill of fever, but of what sort could not be ascertained. She had suffered from no fright, nor any specially cruel treatment, nor had she been with any case of chorea. She is a very bright, intelligent-looking child, with dark hair and eyes.

Her father stated that, about a week before I first saw her, he had noticed a " drag " in her left leg in walking, and that her left arm afterwards became unsteady; he complained also that " she kicked so that he could not sleep in the same bed with her."

The symptoms of the disease in all its fantastic shapes were well exemplified in this child, though they did not include biting of the tongue. The upper extremities were more affected than the lower, the left side being the worst, and the most remarkable feature was the rapidity with which complete supination, pronation, and rotation of the arm were performed.

Most careful examinations, frequently made during her stay in hospital, failed to discover any bruit or other evidences of any functional or organic disease of the heart; and although the pulse at times was quick, it seemed attributable to the general muscular excitement. Anæmic symptoms were not prominent.

Treatment consisted of plain nutritious food and the gradual and persistent use of strychniæ sulphas.

On the 9th January, $\mathfrak{m}$. 4 of the solution of sulph. strychniæ (referred to in last case), equivalent to about $\frac{1}{60}$th grain, were administered in syrup three times a day.

On the 11th it was increased to $\frac{1}{40}$th grain.

On the 12th to $\frac{1}{30}$th grain; and so on each day to the 18th, when 14 $\mathfrak{m}$. of the solution, or $\frac{1}{17}$th grain, were employed.

Improvement did not show itself the first two days of the treatment, rather she seemed worse. However, on the fourth day, while taking $\frac{1}{30}$th grain, she began to show signs of amendment, and each day she got better and better. This continued to the 19th, when, as I have said, $\frac{1}{17}$th gr. was reached. On that day it was

found necessary to stop the treatment, as the child complained of pain in her neck, with stiffness in neck, back, and legs; and when placed on her feet she was unable to stand, being inclined to arch backwards, the toes of both feet were drawn forcibly in under the soles, and she cried out from the pain. All medicine was omitted, and towards evening these symptoms, indicating a full physiological dose of strychnia, had entirely disappeared—and, what was of equal importance, with them to a great extent the irregular movements. Next day it was most evident to everyone that she was very much quieter than ever before; and, except when spoken to, or on attempting to sit up, the choreic movements had well-nigh ceased. I thought it well, however, not to stop the medicine altogether, so the patient recommenced next day the sulph. strychniæ in doses of $\frac{1}{24}$th grain.

On this day I applied the ether-spray for two minutes, a longer application serving but to alarm the patient. I employed it again on 22nd and 27th, and it was followed by quietude in each instance. From the 20th January to 1st February the dose of strychnia was gradually again increased, and $\frac{1}{15}$th grain, ter in die, was reached before its physiological effects again showed themselves to a slight degree. The dose was again reduced to $\frac{1}{20}$th, and continued at that dose for four days longer, when pills of ferrum redact. were substituted. The improvement, which first showed itself in so marked a manner on 20th January, never went back, but rather steadily increased and continued without interruption till February 3rd, when *all* the symptoms, which on admission on January 5th she had exhibited, were completely gone. She was able to command the muscles of the face; could keep the arms and forearms steady; could extend and flex at pleasure her fingers; could walk with ease along a chalked line, and go up and down stairs. She left hospital perfectly well, fat and strong, in a few days afterwards.

She was thus under strychnia treatment for twenty-five days, while improvement showed itself in four days; and the whole duration of the attack was but six weeks and three days.

CASE III.—*Primary Severe Chorea, bilateral; no Rheumatism or Cardiac Complication; Treatment, Ether-spray and Nerve-sedatives; Improvement.*

[Reported by Mr. W. H. Bracken, Clinical Clerk.]

Joseph G., aged nine years, was admitted into hospital in March, 1876, suffering from chorea of about fourteen days' duration.

It appeared that about this time his mother noticed strange twitchings of his head. The irregular movements were next observed in the right arm, and afterwards in the right leg. The left side was then affected, but the movements were never so severe in the left as in the right side. There was no history of rheumatism. On admission there was well-marked chorea of the face, eye-lids, lips, and tongue; the whole body, and extremities, particularly those of the right side, were in constant agitation, so that various parts of his body were erythematous and abraded; the child could not stand, but he was able to walk in a wild, spraddling, plunging manner. The uneducated looker-on, in addition to being struck by its ludicrous appearance, must have expected such muscular exertion to be attended with perspiration and subsequent fatigue. Though well known, it is, nevertheless, a very curious fact, that sufferers such as this case and the others I have described, although they writhe, wriggle, toss and twist from morning to night, do not betray fatigue, nor are the most violent movements attended with perspiration. The patient could speak tolerably distinctly, though the words of the sentences he employed were run into each other with wonderful velocity, and many an ill-timed in-sucking of the breath made them halt and stagger in a curious way. There was no evidence of any organic disease of the heart or blood-vessels.

The only cause which could be learned as at all likely to produce chorea, in the absence of his having received any great fright, or of his having witnessed chorea in other children, was the existence of intestinal worms (*ascarides lumbricoides*), twelve of which had been expelled by santonin, by my colleague, Mr. Croly, who had seen the child before admitting him to hospital.

These entozoa have, by many, been considered a sufficient exciting cause of chorea, but why worms should in one child induce such an affection, and in another produce no nervous symptoms— and why, in this child, they did not induce this peculiar train of irregular movements sooner—I do not feel myself competent to explain, and I do not care to theorise about it.

While in hospital two more female worms were got rid of by santonin followed by castor oil.

In this case I was anxious to see which of the two remedies I employed in my former cases—the strychnia or the ether-spray— was the more to be relied on, and, accordingly, I gave no strychnia for the first ten days he was in hospital, and only employed the

spray to the spine, from the nape to the coccyx. At first the application was for five, and then for ten minutes. It was used every second day. In addition to the ether-spray, which seemed to be always followed by much comparative quietude, it was found necessary to give him sleeping draughts, as for some time before admission to hospital he had not slept, but tossed about all night. The following draught was ordered and repeated nearly every night:—

R. Potass. bromid. . . . gr. 10,
Tinct. hyoscyami . . ɱ 20.
Hydrat. chloral . . . gr. 10.
Syrupi simpl., . . . ʒi.
Aquæ . . . . ʒi. M.

At the end of the ten days the report was that the excited irregular movements are much lessened, and that he can perform, with some degree of ease, some small action, such as picking up a coin, shutting and opening his hand, but that he cannot walk steadily, nor extend and flex each finger at will. He was improved a little. I could, therefore, say that in this case the ether-spray, while it lessened the severity of the movements for a while, did not act curatively. The treatment was now changed, and strychnia commenced, but I lost sight of the patient soon afterwards, as his mother, seeing him somewhat better, took him home. The contrast of this case and the others is remarkable, and points to the advantages of the early use of strychnia.

CASE IV.—*Chronic Chorea; Relapse; Treatment, Strychnia in small doses and Ether-spray; considerable improvement in a week.*

A. M., a young lady, aged fifteen, came under my notice the middle of last year, suffering from a relapse of chorea. She had not as yet menstruated.

Three years before, she had got a severe attack of chorea, brought on by fright, the room in which she slept having caught fire. At that time she was hardly expected to live, so incessant, day and night, were the movements, and she was worn to a skeleton, showing that the functions of organic life were deranged as well as motility.

She, however, recovered, after nine months' illness, though the mind, for some time, showed evidences of impairment of healthy vigour. Since then she had three relapses, and seemed to be never completely free of some of the staccato movements of chorea. In

the early part of 1875, arsenic had been employed with benefit, but the treatment was stopped a month before I saw her. Before coming under my care the symptoms had returned with severity, so that she fell going up or down stairs, was unable to dress herself, tore her clothes by the wild unrestrained movements, while her lips and cheeks were rendered sore and raw from an unending sucking, smacking motion of the mouth and tongue. The emotions were also easily excited. She became fretful and irritable, and did not sleep. The right side of her body was that most affected.

The treatment I adopted was strychnia, and ether-spray to the spine on three occasions, with most satisfactory results, as, after a few days, the violence of the symptoms abated, and she was able to go about the house without danger to herself or injury to the furniture. The dose of sulph. strychniæ was not pressed to its physiological effects, not exceeding $\frac{1}{50}$th grain—as in private practice one has not that constant supervision which one enjoys in hospital practice, and which is necessary, I conceive, to the wise and safe administration of so powerful a nerve-stimulant. Family circumstances made this patient leave town for the country before she was completely cured, when I lost sight of her for a year. I heard, however, that there have been no more severe relapses; and when last I saw her, a month ago, she was completely free of all signs of the disease.

How far this favourable change is fairly to be accredited to this treatment, or to the occurrence of puberty, I cannot decide; but, knowing how the regular establishment of menstruation allays emotional diseases, I incline strongly to the latter being the cause, rather than the former. I merely adduce this case as a fitting sequel to the former, inasmuch as it furnishes an example of the benefits of the treatment by strychnia, even in chronic cases, and when the medicine is given in but small doses.

The foregoing cases are too few from which to draw any general conclusion, either etiological or therapeutic, but it may not be out of place to notice, in passing, how in age they conform to that period which is generally looked upon as a predisponent factor. Chorea is especially a disease of childhood and youth. Thus, of Professor Sée's 531 cases, 453 occurred in children between six and fifteen years of age; of Hammond's 82 cases, 67 were in the same period; and in a valuable digest of 100 cases recorded by Dr. Hughes, in *Guy's Hospital Reports*, Vol. IV., 1846, 70 occurred under sixteen.

I lay no stress on the equal number of boys and girls affected, as it was entirely fortuitous—nor would I call attention to the fact that two were of a dark complexion and dark hair, and two were fair—except to express disapproval of the theory, which some have stated, that one or other variety of colour and complexion operates as a predisponent of this disease.

But I would beg your attention to the fact that, at no time under observation, did any of the four cases show the physical signs of endocarditis or pericarditis, nor was there a history of rheumatism in any one of them. This is a point of considerable importance, bearing, as it does, on the views of certain pathologists who connect chorea with rheumatism of the heart. While I agree with the general truth, that rheumatism and chorea are often associated, I am not prepared to concede the point to those who hold, with Dr. Tuckwell,* " that the absence of cardiac murmur may exist, although the valves be extensively diseased, inasmuch as," they say, " the warty vegetations and growths are upon the auricular surface of the mitral valves," and they would have us believe that all the cases of chorea, which have been attributed to pregnancy, fright, worms, &c., can be really due to these vegetations on the valves becoming in their turn emboli, and thus setting up irritation and softening of the nervous centres.

Of the exciting causes to which chorea is usually attributed, in but two of my cases could any such be fairly adduced, though, doubtless, bad hygienic influences and deficient nourishment aided the production of chorea in the others. The first case may, I believe, be put down to excessive mental occupation by close study, and fright was clearly the excitant in the last.

The average duration of the disease, when left to itself, should be remembered when the treatment of the complaint and its influence on the progress of the case is considered. The tendency of chorea is to increase to a certain point, and then gradually diminish. Hammond considers the usual time to be three months, Niemeyer six to eight weeks, and several published cases—in particular 38 cases recorded by Drs. Gray and Tuckwell (*Lancet*, 1871, Vol. II., p. 814; and 1876, Vol. II., p. 711)—point to nine weeks and six days as its average duration. Hence one must take particular precaution not to fall into the fallacy that any special treatment is curing the patient, when the recovery is no sooner than in the average time of the disease when left to itself. ·

* British and Foreign Quarterly Journal. September, 1867.

It is perhaps for this reason that there is no disease for which so many remedies have been recommended, and of which so many vaunted cures have been recorded, as chorea.

The treatment of chorea ought to fulfil two indications—to shorten its duration, and to temper and moderate the more distressing symptoms; and, should it fulfil these, it deserves to be valued as useful.

Opinions are divided as to the first indication—some, with Niemeyer and Drs. Gray and Tuckwell, holding that medication is totally ineffectual to check the progress of chorea; others, such as Trousseau, Begbie, and Hammond, believing that its course can be shortened by treatment. Dr. Broadbent has expressed in this point a sound opinion. He says:—" I have came to the conclusion that the duration of chorea may be shortened by treatment, but equally am I of opinion that the same treatment does not answer in all cases. I have seen successes with purgatives, tonics, iron, zinc, arsenic, bromide of potassium, cod-liver oil, &c., and I have also seen failures with all the heroic methods of administering strychnia, till its physiological effects were manifested, and of giving tartar-emetic in enormous doses." Agreeing, as I do, with this general statement, I would claim for strychnia that in the two cases in which its administration was fully and regularly carried out, it fulfilled the first indication by shortening the disease, and in three cases it rapidly alleviated the symptoms and quieted the movements. As far, therefore, as these cases go—and I desire not to strain their evidence—strychnia proved to be well suited and the proper treatment.

To come to any definite general conclusion on such a subject, without much and extended observation, would be in the highest degree reprehensible, but it is all the more the duty of each clinical observer to record the results of his experience. It is with such intention I have brought these cases of chorea under the notice of the Society, and it is my hope that their recital may elicit the much more extended experience, on this subject, of those among us who, from lack of time or inclination, could not sit down to detail the cases, which constantly fall under their notice, and the results of their treatment.

The treatment by strychnine, as adapted by Trousseau,[b] has been misquoted in several books, and has unfortunately got the

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[a] British Med. Jour., 1869. Vol. I. P. 371.

[b] Clinical Lectures. Trans. Syd. Soc. Vol. I. P. 418.

opprobrious title of " heroic;" and. probably, this has deterred many from employing it. Trousseau, himself, predicted its disfavour and disuse (p. 419). It has, moreover, been considered not only ineffi-cacious, but absolutely dangerous by Rilliet and Barthez, and G. Sée; and although this latter view is contested by M. Moynier, one of Trousseau's pupils, it is a method which does not altogether commend itself to me, and which falls short of that I have employed. It may be summed up in the words of Moynier:*—" The medicine must be given in doses not necessarily large, but sufficient to pro-duce stiffness; and this rigidity should be attained *as soon as possible*, as the duration of the treatment is much prolonged when the patient has exhibited no rigidity."

The method of administering this powerful nerve-tonic, which I employed in all my cases, was that recommended by Hammond, and I was guided in the dose more by its influence on the move-ments of the patient than by the desire to produce its physiological results, as Trousseau would teach.

Besides chorea, I have employed strychnine in other cases in the manner I have described with excellent results—and in one case in particular, the choreiform movements of cerebro-spinal insular sclerosis were kept better under control by its employment than by other nerve-tonics, or nerve-sedatives.

Administered in this way it is, I am convinced, a safe as well as a most useful remedy. It is not difficult to get children to take it, as its bitterness can always be masked by the various syrups of the Pharmacopœia, and it has the twofold effect of improving digestion and increasing appetite, as well as of keeping the bowels regular—matters of no small moment in the treatment of choreic patients.

Hammond[b] states that he " had never seen the slightest ill con-sequences follow this mode of treatment," and " that he had carried it out in thirty-two cases occurring in children under the age of fifteen, and in three persons of adult years, without a single failure." Such evidence is worthy of all attention, and should be a sufficient answer to those who object to strychnia being used in doses large enough to produce its physiological effects, but I have a higher authority than Hammond on the safety of this medication and its therapeutic powers; for, as you all doubtless recollect, in his last lecture delivered in this hall (Nov. 27th, 1876)—one in

* Du Traitement de la Chorée. Par Eugène Moynier. Archives Gen. de Méd. 1854. T. II., p. 86.

  b Ibid. P. 617.

which he dwelt on the treatment of brain-disease—Dr. Brown-Séquard gave it as his opinion—an opinion which must carry with it all the weight and force of such an authority—that in many cerebral diseases strychnine is the chief remedy to rely upon, and that to obtain its good results it should be employed, and employed fearlessly, to produce its physiological effects—nay, further, that these effects ought to be kept up, so that the slight tetanic rigidity of the muscles should be maintained for four, five, or six weeks.

ART. IV.—*On the Morbid Changes in the Nerve-elements of the Brains of the Insane.* By RINGROSE ATKINS, M.A., M.D., &c.; Assistant Medical Officer, District Lunatic Asylum, Cork.

PREVIOUS to the close of last session I had the honour of communicating to the Association the result of my investigations on the morbid changes occurring in the blood-vessels of the brains of the insane, and I then stated that I hoped to continue these investigations into the lesions taking place in the nerve-elements under similar circumstances, and, at some future period, to be able to demonstrate before you the various conditions I might observe. Accordingly, during the past year I have, with this object, examined minutely a considerable number of the brains of those dying insane, in varying phases and different stages of the disease, and the results obtained therefrom are embodied in the present paper; the actual conditions being demonstrated by the specimens and drawings submitted for your inspection. The cortex of the brain, as you are all probably aware, consists, especially in man, of a rather thick layer of grey nervous matter, spread like a cap over the entire upper surface of the hemispheres, and, indeed, everywhere over their surface, except a comparatively small space at the base of the brain, where it is absent to admit of certain masses of nerve-fibres passing in and out of the interior of the organ, and which fibres serve partly to connect the cells of the inner surface of the cortex with the masses of grey nervous matter of the medulla and spinal axis below it. The superficial area of this great cap of nervous matter is much increased by being thrown into numerous folds or convolutions, both longitudinal and transverse, which, if it could be blown out like a bladder or a balloon and the folds thus

* Read before the Cork Medical and Surgical Association, April 12, 1876.

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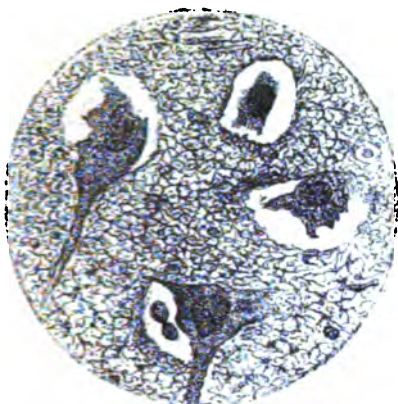
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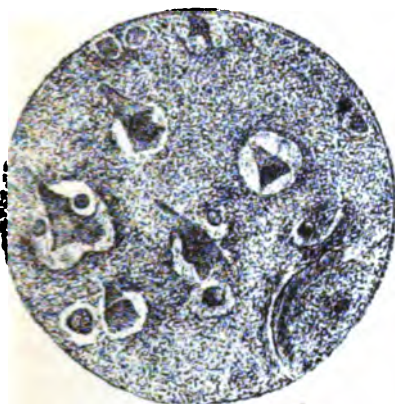
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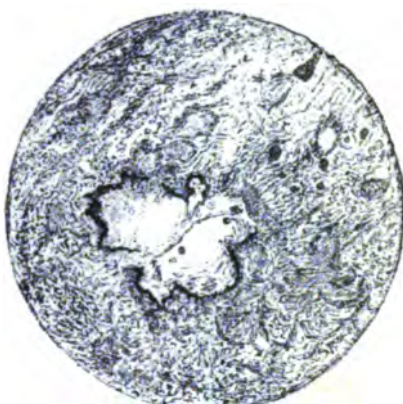
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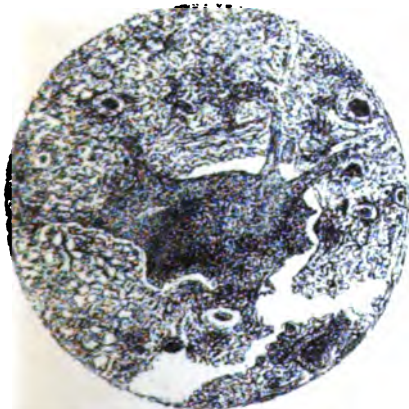
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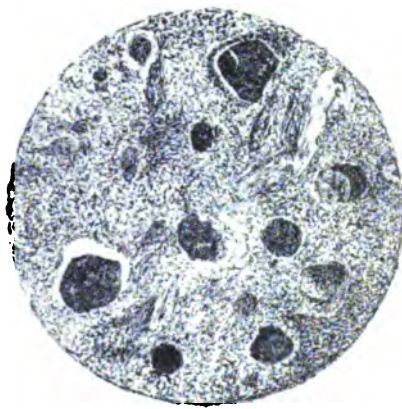
EXPLANATION OF PLATE.

I.—Section through parietal lobe, showing a group of large nerve-cells, the edges of which are breaking down from degeneration of the protoplasm itself, stained with aniline blue-black. Hartnack, ob. 8, oc. 3. (The cells figured have been brought near each other to include them in the drawing ; in the section they did not appear in one "field.")

II.—Section through parietal lobe, showing simple atrophy of the cells, stained with aniline blue-black. Hartnack, ob. 8, oc. 3.

III.—Section through parietal lobe, showing "giant cell," stained with carmine. Hartnack, ob. 8, oc. 3.

IV.—Section through corpus striatum, showing the reticulated condition of the neuroglia, stained with carmine. Hartnack, ob. 7, oc. 3.

V.—Section through medulla oblongata, showing a spot of "miliary sclerosis," stained with carmine. Hartnack, ob. 7, oc. 3.

VI.—Section through parietal lobe, showing a group of amyloid bodies scattered in the nerve-tissue, stained with carmine. Hartnack, ob. 7, oc. 3.

obliterated, would occupy a much larger space than that afforded by the cavity of the cranium. When carefully examined this cap of grey matter is seen to consist of numerous layers, varying from three to eight, with intervening white layers, the latter consisting mostly of fine white fibres passing in various directions. The grey layers consist, as you know, of all but innumerable cells of angular, pyramidal, and other forms, very small and variable in size, and connected together, in a remarkable manner, by means of fine fibres and fine branching processes from cell to cell. These cells differ not only in form and size in different layers, but in different parts of the brain in corresponding layers—the parietal and frontal regions possessing the large pyramidal cells in greatest number, while the round and oval ones predominate in the occipital, according to an observation of M. Betz. The probable total number of the nerve-cells is something immense, there being about 800,000,000 in the grey matter of the cortex alone, as has been computed by M. Luys. These cells are embedded in a translucent granular substance—the neuroglia, are separated and supported to a certain extent by delicate bands of connective tissue, and the whole is bathed in a plasma from a rich network of small vessels (Jewell). Each cell consists of a mass of protoplasm destitute of a cell wall, and presenting, according to Max Schultze, a granular fibrous character. The younger the individual and the more natural the state of the brain examined, the more frequently does the contained nucleus possess an outline similar in shape to the mass of protoplasm, be it angular, pyramidal, or fusiform, projections from these nuclei even passing into the origins of the branched processes proceeding from the bi-polar or multi-polar cells. Arndt, however, believes that the nuclei are originally vesicular, and that they have been squeezed by the masses of protoplasm in accordance with the shape of the latter. The nucleoli are circular, lustrous, and in successful carmine preparations of healthy tissue, as a result of colour contrast, are surrounded by a bluish-green halo. Besides the angular, pyramidal, and fusiform cells, there exist in innumerable numbers spherical particles, many of which are smaller than a red-blood corpuscle, and probably connected together with communicating fibres of great delicacy. These bodies are situated near the surface of the grey matter among the ultimate sub-divisions of the excessively delicate nerve-fibres, they are spherical after death, and unenclosed by any cell-wall, and retain their primitive characters throughout life (Beale). The two drawings which I now exhibit

represent a group of large pyramidal cells, and a number of the smaller bodies. The preparations from which these drawings are taken were made by Mr. Sankey's process (*vide* "West Riding Asylum Reports," Vol. V.), the sections being coloured with aniline blue-black, and cut from the fresh brain. The delicately fine supporting material in which the various forms of cells are embedded, and regarding the nature of which differences of opinion still exist, and to which different appellations are yet applied, consists normally of two elements—(1) a network of fibroid trabeculæ, containing in its meshes (2) a finely granular amorphous material. The fibroid trabeculæ form, by their junction, a reticulated meshwork, and at the points at which the individual trabeculæ meet protuberances or knots are formed. In the centre of these knots, especially of the larger, is a rounded or oval body deeply coloured by carmine; these bodies are nuclei finely granular, destitute of a nucleolus, and soluble in acetic acid; they are known by the name of *Myélocytes* (Robin), or nuclei of the neuroglia (Virchow). These neurogliar nuclei are merely surrounded with a little protoplasmic material, or contained in a star-shaped cell from which prolongations continuous with the trabeculæ pass (Deiter's cells). Sometimes these "cells" become so developed that it is difficult to distinguish them from true nerve-cells. In the grey matter this fibroid reticulum is very dense and the meshes are more closely packed, and this condition also obtains to a great extent in those parts of the white matter where no nerve-tubes exist. The finely granular amorphous material is more easily demonstrated in the fresh state when the fibroid reticulum does not come into view; it appears as a soft, greyish, finely granulated amorphous material, in the midst of which the *Myélocytes* are suspended. The difficulty of demonstrating the fibroid meshwork has led some authors, as Henle and Robin, to deny its existence. There can be no doubt, however, from the fact that in certain pathological conditions where the normal state of things become simply exaggerated without being modified, this reticulum can be readily demonstrated, and hence it does in reality exist in the healthy tissue. That this complex structure of cells and fibres is the physical organ of the mind, the material substratum of our mental operations, comparative physiology, the freaks of nature, and the experiments of disease have taught us beyond the possibility of a doubt; and that every mental act is associated with some molecular change in the grey matter of the convolutions is a

proposition universally admitted by physiologists. What this "molecular change" consists in, or how the thoughts are evolved, and the mandates of the will flashed with lightning rapidity along the nerve-fibres to their furthest peripheries, are questions still wrapped in mystery. Granting, however, the all-important fact that healthy mind depends on healthy brain, does not the converse of the proposition, that a diseased mind depends upon a diseased brain, force itself upon us almost with a claim of reason, and hence, seeing, as we do, the gradual unravelling of the pathology of the diseases of the nervous system as the result of minute investigation, does it not behove us, not only as a matter of scientific interest, but also of practical importance, to direct special attention to the endeavour to determine the nature and origin of the lesions which produce such baneful effects on the mental apparatus, as those specially engaged in this department see daily brought under their notice, with the hope that we may ultimately be able to recognise with precision early departures from normality, prior to the irremediable changes of structure, and so apply our remedies rationally and with a more certain hope of success?

In my former paper I discussed the all-important influence which morbid conditions of the circulatory apparatus hold over the integrity of the nervous tissues; and it is my intention now to endeavour to demonstrate to you, and discuss the pathological significance of those subsequent changes to which I then alluded. Whether the nerve-elements ever become primarily affected is a question which still remains unanswered. Speaking from personal experience, I have never yet seen a brain in which I could say the nerve-elements were primarily implicated, while the earliest changes which I have been able to demonstrate have invariably been in the vessels. True, indeed, I have not had an opportunity of examining a brain from a case presenting symptoms of a primary dementia, in which, Dr. Batty Tuke states, a diseased condition of the neuroglia depending on morbid processes, commencing primarily in its own constituents, will always be found. In these, however, vascular lesions are also present, and hence it becomes a matter of great difficulty to determine in which situation the departure from normality began. In certain forms of idiocy also, conditions of the nervous tissues may be observed, whose dependence on prior vascular lesions is in like manner doubtful. In describing the specimens which I now submit, and in discussing their pathological relations and significance, I will deal—first, with the morbid conditions of the

nerve-cells, as possessing the most important functions; secondly, with the changes found in the *neuroglia*; and thirdly, with those occurring in the *nerve-fibres*. This I do without prejudice to the priority or extent to which each of the structures first mentioned may become affected. Were I to follow the latter order I would be inclined to consider the neuroglia first, as it is in this tissue, I believe, the most general, if not the most definite or easily demonstrable lesions take place.

I. *Morbid conditions of the Nerve-cells.*—The diseased conditions observed in the different layers of cells entering into the structure of the cortex of the brain may be classed under two heads:— 1. Lesions of individual cells; 2. Alteration of their normal situation and relation to each other. To the first belong the various forms of degeneration and atrophy, while the second includes that alteration in their normal position and relations that "dislocation"— if I may use the term—which the cells undergo consequent on morbid conditions in the neuroglia. Considering first the lesions of the individual cells, we have, as most frequently met with:—

1. Pigment atrophy, or "fuscous degeneration," as it has been designated by Drs. Batty Tuke and Rutherford, was first described by Dr. Franz Meschede (Virchow's "Archives," 1865, abstracted by Dr. Blandford, *Journal of Mental Science*, October, 1866) under the term "fatty degeneration" of the cells, as one of the organic lesions occurring in general paralysis of the insane. In a paper in *The Lancet*, September 1st, 1866, Dr. Lockhart Clarke also mentions its presence in this disease, and since then Dr. Howden and the two authors whose names I first mentioned have called attention to it in various forms of insanity, and a similar condition has been minutely described under the term "granular degeneration," by Dr. Herbert Major ("West Riding Lunatic Asylum Reports," Vol. IV., 1874) in senile atrophy of the brain. The cells in this lesion are observed to pass through three stages or degrees of degeneration—1st, that of *infiltration* or *imbibition;* 2nd, that of *precipitation;* and, 3rd, that of *disintegration.* In the first stage the protoplasm of the affected cells becomes infiltered with a bright yellow refractive material, which may occupy either the body of the cell, being deposited around the nucleus, displacing this body and leading to a bulging of the contour of the cells, or it may be confined to either extremity of the latter, giving rise to a "bulbous" or "clubbed" appearance; or, again, it may be so

diffused as to cause a generally swollen condition of the protoplasm; the processes proceeding from the basal extremity of the cells are commonly diminished in number, and cannot be traced for any length. The nucleus is usually distinctly seen, though somewhat altered in shape, losing its angular appearance, becoming ovoid and even rounded, and dotted with fine granules. The nucleolus is also generally distinctly visible, being deeply coloured by the staining reagent, but is not apparently otherwise morbidly affected. The drawing which I now exhibit, taken from a section through the corpus striatum, illustrates this stage of the degenerative process; a group of large fusiform cells are seen infiltrated with a yellow material which, in some of the bodies, appears as a mass of finely-divided particles collected at either extremity of the cell or around the nucleus, refracting light and uncoloured by the carmine with which the section is stained. This deposition of molecules ushers in the second stage—namely, that of precipitation. In this stage the yellow material becomes separated into masses of granules, at first retaining their yellow colour, but as they become older and as the degeneration advances, assuming a dark brown or even blackish appearance. These granules are deposited either in the interior of the protoplasm, displacing the nucleus and frequently extending into the large nerve processes proceeding from the cell, or on the surface of the latter, completely obscuring the nucleus, which can, however, be again brought into view by altering the fine adjustment of the microscope so as to bring the deeper portions into focus, thus showing that the nucleus is not destroyed but merely obscured by the granules lying above it. The basal processes at this stage usually become entirely lost, and this extremity of the cell losing its angles becomes rounded off and club-shaped; the apical process can no longer be traced for any length, and often ends in a blunt point. In the third stage the protoplasm becomes completely disintegrated, the cell, which has lost its shape and size, being represented by a mass of blackish-brown granules, loosely aggre-gated, and apparently ready to fall asunder, surrounded in whole or in part by a clear space indicating that atrophy as well as degeneration has taken place, that the material now remaining is less in quantity than the original cell which it has replaced. In many of these loose masses of granules no trace of a nucleus can be discovered, while, again, in what appears to be the most advanced degree of the lesion, the nucleus, generally almost circular or ovoid, can be seen lying naked in the surrounding tissue or with merely a

few large granules still adherent to its circumference, the nucleolus being generally invisible. .

These second and third stages are well illustrated in the specimens from which the two drawings I now point to are taken, the one representing a group of cells in the corpus striatum of another brain; the other a number of cells from a fresh preparation of a portion of the cortex from one of the parietal convolutions. In the first, several of the cells are represented by masses of dark-coloured granules, contrasting with the bluish-violet of the aniline blue-black with which the section is stained; in one large cell the polar process is yet undestroyed, and the granules, still yellow, can be seen deposited along it. The second drawing shows several cells similarly affected, proving that whether examined in the recent or prepared state this lesion in its different stages is identical in both. Scattered in the walls and in the perivascular spaces surrounding the minute arterioles in the immediate neighbourhood of the affected groups of cells in these sections, are numerous granules of hæmatoidin and collections of molecules strongly resembling in colour and appearance those which have taken the place of the disintegrated cells. This condition of pigment atrophy or degeneration is found most commonly in the large fusiform and pyramidal cells, occurring in the frontal and parietal convolutions, the corpora striata, optic thalami, pons Varolii, medulla, and spinal cord. Dr. Batty Tuke states that it has not hitherto been observed in the cells of Purkinje. I have, however, recently examined a softened portion of cerebellum where over the surface of these bodies more than a normal quantity of granules were scattered, not sufficient, however, to entirely obscure the nucleus or nucleolus, or to cause destruction of the cells which had, however, lost their processes and become rounded. The small pyramidal and angular bodies forming the superficial layers of the cortex, and the rounded cells accompanying them in the occipital lobes, are rarely found affected with this form of degeneration, and I have never seen them reduced to the masses of granules such as I have described as occurring in the larger bodies. This lesion is not peculiar to any one form of insanity; it has been found extensively in general paralysis (Meschede, Clarke, Pointcaré, and Bonnet, Westphal), senile atrophy (Major), and melancholic mania (Batty Tuke); it occurs, I believe, most extensively in long-continued cases of insanity where defective nutrition of the cells is persistent, but I have seen it to a marked degree in a case of mania terminating fatally in a twelve-

month, as well as in the brain of an epileptic where life had
terminated during youth. Regarding the nature and origin of this
lesion differences of opinion exist. Drs. Lockhart Clarke and
Meschede considered it to be a fatty degeneration, while Dr.
Howden, who found the molecular masses to be unaffected either by
sulphuric ether or strong alkalies—an observation which I have
repeated with a similar result—holds an opposite opinion, in which
Dr. Batty Tuke coincides. That the morbid condition is of
pigmentary origin is, I think, highly probable from the following
considerations:—

1. That colouring matter, when it has escaped from the blood
corpuscles, has a special tendency to become deposited on " cells,"
the protoplasm of the latter attracting it far more powerfully than
the intercellular substance, the nuclei remaining unaffected for a
considerable period.

2. The further changes which such colouring matter undergoes
when effused into cells—viz., the separation into molecules, &c.,
being almost identical with what we observe taking place in this
lesion.

3. The almost constant occurrence, with this form of degene-
ration, of crystals of hæmatoidin and fine granules on and around
the vessels, a form of blood pigment much more common in this
situation than in the cells.

4. The difficulty with which the diffused material or the masses
of granules become coloured by the staining fluids used in the
preparation of sections or in the demonstration of fresh specimens,
especially the latter.

5. The yellow colour of the granules when newly deposited,
gradually becoming darker as they grow older, thus resembling
hæmatoidin; and

6. Conditions favourable to the extravasation of blood pigment
being generally present where this form of degeneration is met
with.

These facts, taken with the negative results obtained from the
application of the chemical tests for fatty or oily substances, warrant
us for the present in assuming that the yellow material infiltrating
the cells is of the nature of *hæmatin.* Let us hope, however, that
the more delicate application of chemistry, or perhaps of spectrum
analysis, to such investigations, may sooner or later definitely answer
this as yet unsettled question.

In addition to this "pigmentary," I have observed another form

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of degeneration of the cells differing from it in appearance, and I believe also in nature and origin. This lesion is characterised by the gradual erosion and breaking down of the protoplasm from around the nucleus, leaving that body naked and free in the surrounding tissue, and is dependent not on the damaging influences of any material imbibed from without, but on changes inherent in the cell substance, which lead to its disintegration and ultimate destruction. The first degree of this morbid condition appears to be a diminution or total disappearance of the nerve processes, arising from the basal angles of the pyramidal or angular cells, and a blunting or complete rounding of these extremities, the apical process still persisting; as the lesion advances the outline of the cell becomes blurred and irregular, and the entire protoplasm assumes a coarse appearance, without, however, there being any indication of pigment deposit or distinct granules, the nucleus becomes oval, and readily takes up the staining fluid, and the nucleolus can also be seen; little masses now break away from the edges of the cells, leaving the latter ragged, and gradually this erosion of the protoplasm proceeds to such an extent as to cause the total loss of shape of the affected bodies, finally leaving the nucleus free or with little shreds of the protoplasmic material still adhering to it. The drawing I now show you (Plate, Fig. I.) represents a group of large cells thus affected, presenting the rounded basal extremities, and the eroded and broken down appearance—differing, however, entirely from the loose masses of granules I have already described. This gradual breaking down of the cells may not proceed to their complete destruction; the affected bodies before the degeneration is complete sometimes shrivel up, and become quite opaque, and coincidently with this I have observed the apex of the cell, or the remains of the process arising from it, broken sharply across, as if the protoplasm had become brittle from a commencing metamorphosis of its structure. This condition may pass into that of complete calcification, as has already been observed by several authors. Dr. Blandford (*Insanity and its Treatment*, p. 104), and Dr. Maudesley (*The Physiology and Pathology of the Mind*, p. 404) mention the occurrence of a calcareous degeneration of the ganglion cells, which are rendered opaque by granules of phosphate of lime. Erlenmeyer found calcified cells in the brain of a monomaniac who had died with epileptiform convulsions. Heschl met with what he called ossification of the cells in the brain of a melancholic; and Dr. Wilks believes certain bodies which he found in the brain of a

general paralytic, in whom the small arteries were calcified, to have been ganglionic cells that had undergone calcareous degeneration. I have observed in a softened portion of the cerebellum of an aged person, for many years insane, a number of bodies about the size and general outline of the cerebellar corpuscles, varying in colour from a clear white to a reddish brown, and unaffected by the staining reagent, the examination being made in the fresh state. In appearance their structure resembled that of a calcareous substance; the edges of many were minutely fissured, and they were frequently broken across with an even transverse fracture, and appeared to contain irregular earthy-looking masses, and in several an indistinct nucleus-like body could be seen. Whether these, however, were undoubtedly metamorphosed ganglion corpuscles I am unable to say, never having met quite similar looking bodies before, and if such is the case their occurrence must be very uncommon. Passing from these forms of degeneration, with loss of substance and final disintegration of the cell substance, we meet with simple atrophy or shrinking of these bodies. While preserving their general outline and characteristics the cells thus affected become considerably diminished in size, acquiring a triangular shape, and being surrounded by a circular or oval clear space, the three angles either touching or passing over the edge of this space. The nucleus does not seem to participate in the shrinking, appearing as if swollen, and occupying a relatively larger portion of the cell than is normally the case. This condition is observed in the section from which the drawing I wish now to direct your attention to is taken (Plate, Fig. II.), the triangular atrophied cells are seen lying in circular clear spaces, and the relatively large nucleus appears as if filling up almost entirely the withered bodies. In the most advanced stages of this lesion the cells may become so diminished in size as to fall out of the containing spaces in prepared sections, and the minute loculi so often seen in the outer layers of the cortex—to which this change is almost exclusively confined—in cases of senile atrophy and chronic brain wasting, are the result of this loss of the cells. These latter forms of degeneration and atrophy appear to be due to a deficient supply of pabulum to the cells which their constant activity calls for, and by which the molecular changes taking place in the exercise of their functions can be carried on and renewed; hence, such deprivation must lead to an annihilation of their functions, and a retrograde metamorphosis of their structure, ending in atrophy or complete

disintegration.   There is still another apparently morbid condition
of the nerve corpuscles, of which I have as yet met with but
a single example; this consists in a " hypertrophy" of the cells, as
it is termed by Dr. Batty Tuke, and Rutherford, who first described
it in a paper read before the British Association in 1870.   It has
been independently discovered by Dr. Herbert Major, in a case
of general paralysis, and afterwards in senile atrophy (" West
Riding Asylum Reports," Vols. III. and IV., 1873 and 1874),
and at first named by him "inflation of the cells."   The single
example of this change which I have met with occurred in the
brain of a man who had been insane for fifteen years, and had
had an attack of left hemiplegia a year before his death.   A
section through one of the parietal convolutions—which is here
for your inspection, and of which this drawing is a minute
portion—shows under a low power (100 diams.) a row or band
of cells of much larger size than usual, situated about the position
of the fourth layer of the cortex, and traceable in a curved
direction throughout the entire specimen; each cell, deeply coloured
by carmine, stands prominently out, and forms a marked contrast
to the normally sized ones which lie about them.   The single
cell represented in the drawing (Plate, Fig. III.), which presents
characters quite similar to the others comprising this remarkable
band, magnified 400 diameters, occupies a considerable portion of
the field of the microscope, and has the remains of eight nerve
processes proceeding from it, four of which project some little
distance, and terminate in broken blunted extremities, the other
four being destroyed at their origin from the body of the cell; the
latter is of an ovoid shape, its protoplasm is finely coloured by the
carmine, and dimly granular; no trace of a nucleus or nucleolus
can be seen by any light, or with any focussing.   The other cells
of the band differ a little in shape, and vary in size, and in several
the nucleus and nucleolus can be indistinctly seen.   The abnor-
mally-sized cells here observed differ considerably in appearance
from those figured by Dr. Herbert Major (" West Riding Asylum
Reports," Vol. IV., 1874) in his specimens; the enlarged bodies,
beyond being increased in size, presented nothing morbid; the
protoplasm was perfectly normal, the nuclei tolerably well defined,
the nerve-processes projecting clear and spider-like for a consider-
able distance, and dividing into their many ramifications.   Dr.
Major states that, although the examples he has depicted, beyond
their increased size, show no other change, yet similar specimens

frequently present in their interior masses of granules, showing very clearly degeneration of structure. This has been the case in the specimens now under consideration, the general characters presenting the appearances of "pigmentary degeneration" in its earliest stages, a little yellow material being observable around the nuclei where these bodies can be seen. Regarding the nature of these " giant cells," whether they are overgrowths of normal structures, or owe their origin to some malformation of type in the development of the cerebral elements, and are in other respects normal, the healthy condition being seen in such specimens as those figured by Dr. Major, and which, like their smaller fellows, may become affected with degenerative changes, as exemplified in those here seen; or whether, again, they are in either condition, primarily, departures from normality, there is no evidence to show, and, consequently, their pathological significance (if any) is unknown. Looking merely to their size and appearance, the term "hypertrophy," proposed by Dr. Batty Tuke, would seem to be correct; not, however, having data from which we can draw any conclusion, it is, I think, better, until such be forthcoming, to use the term " giant," as expressive of the one prominent feature, be it from an increase in the quantity of the protoplasm of the cell or from an inflation of the latter, as a result of an infiltration of some material into it. Such, then, are the morbid conditions of the individual nerve-cells, and, in the various changes which they are found to undergo, it will be observed that the nuclei generally remain intact, or, at least, do not suffer such total destruction as occurs to the protoplasmic material. It is, I believe, the experience of all observers, that the nucleus is the most persistent part of the cell, and these bodies, when they are freed by the disintegration of their enclosing material, instead of being involved in the general destruction, become more or less shrivelled, and assume a somewhat triangular outline, lying naked in the surrounding tissue, and in this state it is often difficult to recognise their true character. Whether fission of the nuclei occurs is, I believe, still an undetermined question. I have frequently seen groups of small bodies resembling nucleoli closely aggregated, and often deposited in a clear space, which at one time appeared to have been occupied by a cell, or lying about the basal extremity of one which still exists; whether these, however, are the result of a fission of the nuclei, or are collections of independent structures, I am unable to say. I have never observed any of these bodies

joined together, or seen a nucleus undergoing the so-called "hour-. glass" contraction, and, hence, it is not likely that fission does occur, as its earlier stages ought to come under observation as frequently, or nearly so, as its most advanced degrees. L. Popoff (Virchow's *Arch.*, XIII., 421, and *Centralblatt*, No. 36), in acute changes taking place in the brain in typhoid and typhus fevers, describes a division of the nuclei of the ganglion cells; but, as far as I am aware, such a condition, while the cells still exist, has not yet been demonstrated to occur in the brains of the insane.

Having thus considered the morbid changes occurring in the individual nerve-cells, we have next to deal with the alterations taking place in their normal relations to each other. A fine transparent section, through a convolution in the healthy state, shows, under a moderately low power, the beautiful fan or fountain-like arrangement of the pyramidal cells, their bases to the white substance, and their apices toward the horizontal layer of the grey matter, and the neuroglia, so delicate that it requires a very high power to demonstrate its structure.

In the diseased state this is all changed; the cells are distorted as to their direction and general arrangement, the different layers are compressed and their elements mingled, as it were, with each other; the neuroglia is coarse and granular, its fibrillar structure abnormally distinct, or its protoplasmic material loose and broken up, and the entire section presents a totally different aspect to that which prevails in the healthy condition. The alteration in the position of the cells is evidently due to this compression or want of support on the part of the neurogliar tissue, which acts the part of a packing material for the other constituents. By the atrophy or shrinking of the superficial portion of the cortex, one or more layers of cells may either totally disappear or become approximated to those beneath, and an alteration in the direction of the axis of these bodies—a dislocation, as it were—may follow, disturbing their intricate connexions and rendering them in the end functionally useless. I have now to consider in detail the nature of the general and special lesions occurring in the neuroglia. This tissue, as I have aleady mentioned, consists of two elements—a fine fibrillar stroma of connective tissue, capable of demonstration only with very high powers, and an amorphous protoplasmic material filling up the interspaces of this delicate stroma. In the brains of the insane both these elements are found diseased; a general increase in the amorphous material is very frequently met with, displacing and

obscuring the fibrillar stroma, and rendered evident by the coarse and often broken-down appearance which sections of the tissue affected with it show, as can be seen in this series of specimens which I have prepared from the brain of a female epileptic. This condition is, I believe, due to oft-recurring infiltrations of plasma from the blood-vessels and lymphatic sheaths, and it is not improbable that to it may be referred the firm bulging brain so frequently met with in long-standing cases of epilepsy. A similar increase in the fibrillar element also, though more rarely, occurs, giving rise to a distinct meshwork in which the connective fibres are more distinctly seen and coarser than in health, and forming, by their interlacement, minute loculi; this condition is generally accompanied by an atrophied state of the protoplasmic material, which can with difficulty be distinguished, occupying the little meshes. In the section from which the drawing I now show you is taken (Plate, Fig. IV.), coarse fibrous bands, somewhat deeply coloured by carmine, can be observed partially mapping the field of view into irregular spaces, these spaces are again divided into a delicate meshwork by much finer trabeculæ less deeply tinted by the carmine, and, at the points where these latter meet or cross each other, irregularly-shaped nuclei are deposited, from many of which slender processes can be distinguished passing with the trabeculæ in various directions. This condition is allied to a true *sclerosis*, and, as a general change, is much less commonly met with in the brains of the insane than the coarse and granular condition already described. Passing now to the special and more localised lesions of the proper structure of the neuroglia, we meet with what has been termed "disseminated (insular) sclerosis," or sclerosis occurring in patches. This, in my experience, is also a rare lesion where insanity is uncomplicated with certain physical indications of disease of the nervous centres—in fact, the peculiar train of symptoms resulting from this morbid change are now as well known perhaps as any of the more uncommon forms of organic disease of these centres, and its presence can confidently be predicted during life. The most frequent seats of disseminated sclerosis are the pons, medulla, and spinal cord; although it occasionally occurs in the white matter of the hemispheres. The patches appear to the naked eye as light grey tracts of various shapes and sizes, which are quite opaque and cannot be rendered transparent by any known reagent; they generally occur in contiguity to blood-vessels, and are accompanied by great proliferation of nuclei on and around the latter.

M. Charcot, who has, perhaps, given the best description of the histological characters of this lesion, says that under the microscope a sclerotic patch is not distinctly defined, the diseased parts gradually merging into the healthy; three zones, or degrees of advancement of the disease, can be distinguished in such a patch— viz., (1) the peripheric zone; (2) the transitional zone; and (3) the central region. In the *peripheric* zone the trabeculæ of the reticulum are thickened, the nuclei either larger than normal or increased in number, and the nerve-tubes are more distant from each other, and atrophied at the expense of the medullary sheath, and the amorphous material filling the meshes is more abundant than normal. In the *transitional* zone the trabeculæ are somewhat transparent, and in part replaced by bundles of fibrils placed parallel to the nerve-tubes, which are here represented only by the axis cylinder, which appears larger than in health; the above-mentioned fibrils encroach on the nerve-tubes, and appear, on transverse section, as little dots, while on longitudinal section they are seen as fine opaque fibres arranged in parallel fasciculi, slightly undulating and interlacing, but rarely anastomosing; they differ from the axis cylinder of the nerve-tube by being tinted with difficulty by carmine, the latter absorbing it readily. In the *central* zone the trabeculæ have disappeared, being entirely replaced by fibrils which now occupy the entire tissue, filling the spaces before occupied by nerve-tubes, which have now disappeared except the axis cylinder; the nuclei have shrunk and appear in little groups between the bundles of fibrils, and are but little coloured by carmine. This fibrillar metamorphosis is a special feature in sclerosis in patches. It is difficult to say whether these fibrils are really the products of metamorphosis or a substitution; they appear to spring from the substance of the nuclei or of the cells. The change taking place in the vessels consists in a gradual thickening of the coats from the peripheric zone inwards, by layers of fibrils similar to those developed in the reticulum and the multiplication of the nuclei; in the central zone the walls become so thickened that the calibre of the vessels is much diminished. This lesion is not improbably a localised and intensified form of the more indefinite and general change I last described; and the appearances which it presents, as described by M. Charcot, differ somewhat according to the peculiar anatomical conformation of the special part affected.

The next lesion we have to consider has been termed "miliary

sclerosis" by Drs. Batty Tuke and Rutherford, its first discoverers, from the resemblance which the affected spots bear in minuteness to millet seeds, in comparison with the larger patches of disseminated sclerosis which I have just described. To the naked eye this morbid condition presents itself in a transparent section as opaque specks, scattered either singly or in groups through the affected tissue; when examined with a low power these specks present themselves as minute colourless spots of a pearly lustre, embedded in and displacing the constituents of the structures in which they are seated. Viewed by oblique light the surface of these spots is uneven, and they appear as if raised above the level of the section. Under a high power each spot, as is represented in this drawing (Plate, Fig. V.), taken from a section through the medulla oblongata of an epileptic dement, presents a ground-glass appearance, and is seen to consist of a finely molecular material crossed by delicate fibrils, which can be demonstrated by careful focussing, and separated from the deeply-stained zone of tissue surrounding it by an irregular, puckered, and broken outline. These spots are most frequently single, though they are met with, and I have seen them, aggregated in groups or in a row forming two, three, or more loculi; they are, for the most part, irregularly ovoid or circular, and vary in size, according to Dr. Batty Tuke, from the $\frac{1}{30}$ in. to the $\frac{1}{100}$ in. for the multilocular, and the $\frac{1}{200}$ to the $\frac{1}{800}$ in. for the unilocular patches. They are generally colourless, but sometimes present a greenish yellow tint from the chromic acid or bi-chromate solution in which the specimens are hardened. Miliary sclerosis is believed to be a disease of the nuclei of the neuroglia, and its progress is marked by three stages. In the first, one or more nuclei become enlarged and throw out a viscid plasm of a milky colour which displaces the fibres around them; sometimes several of these nuclei unite, and thus a multilocular patch is formed. In the second stage this plasm becomes distinctly molecular, and permeated by fibrils; and, in the third, the molecular matter becomes more opaque and contracts on itself, the boundary taking on an irregular and puckered appearance, and finally, in the preparation of sections, the material may fall out leaving more or less ragged holes in the tissue. When in this condition the morbid products of miliary sclerosis are distinctly gritty, and effervesce immediately on being subjected to the action of nitric acid.

The pathological significance of this lesion is as yet almost unknown; it has been observed in various forms of insanity, and that

it is not peculiar to the insane has been proved by Mr. Kesteven, who has demonstrated its occurrence in many different forms of nervous disease, such as tetanus, sub-acute myelitis, progressive muscular atrophy, cerebral abscess, glioma, &c., &c. That it is a morbid change in the nerve-tissue is, however, certain, and as such it is of considerable interest and importance, and therefore demands attention at our hands. Frequently associated with this lesion, and often occurring alone, a considerable increase in the nuclei of the neuroglia is observed without any marked change in its fibrillar stroma. The proliferated nuclei appear in vast numbers, filling the surface of the field of view, either singly or aggregated in groups of two or more, those scattered on the arterioles being also affected; they absorb carmine readily, and appear irregular in outline, and often finely granular. The drawing I now show you illustrates this condition, which is analagous to the proliferation of the connective tissue corpuscles in the pia mater, a marked example of which I have here for your inspection and comparison with the section from which the drawing first alluded to has been taken. This increase in the number of the nuclei, which in my experience is of not at all uncommon occurrence, indicates a condition of irritation of the nerve-textures in which these bodies lie, and must be considered an important lesion in regard to the normal state of the structures involved. Besides the sclerosis, and proliferation of the neurogliar nuclei, there is yet another morbid condition, now known as " colloid degeneration." In its earliest stages this lesion shows itself in circumscribed translucent spots scattered irregularly over the surface of the section, varying in size from that of a white to that of a red-blood corpuscle. In prepared sections these spots have well-defined irregular outlines, and their contents are molecular in appearance; in fresh specimens this molecular condition is not observable, the colloid bodies appearing as round or oval in form having a distinct wall, and uncolourable with carmine, occasionally showing a small nucleus, but no nucleolus. In extreme cases these bodies become so numerous that Dr. Batty Tuke, who has most carefully investigated this lesion, compares a section containing them to a slice of cold sago pudding. I have never met with colloid bodies existing in such numbers, but have observed them scattered in the lymphatic sheaths around diseased vessels, and sparsely in the white substance, where they appear at first to commence extending gradually both outwards and inwards. Dr. Tuke considers that the nuclei are the original seats of this disease also, as in all

cases where colloid degeneration presented itself, they are to be seen more or less departing from normality, though not proliferated. Colloid bodies may be distinguished from spots of miliary sclerosis by their clearly-defined margins, the latter presenting more or less broken borders from the ends of fibres and vessels encroaching on their space. Colloid bodies do not undergo the same gradations of development as miliary sclerosis, they do not push aside the fibres, and they never can be removed as a separate substance from the dried sections in which they exist. This lesion has been demonstrated in the brains of idiots, and Dr. Tuke considers that it occurs independent of primary vascular disease, and that it will generally be found in cases of acute primary dementia. Lastly, amongst the diseases of the neuroglia the presence of amyloid bodies scattered through this tissue must be noted. Virchow considers these bodies to be normal, or at least the products of natural decay; while this may be possible under certain circumstances, there can, I think, be no doubt that in many cases, from the number and state of aggregation in which they are found, that these so-called amyloid corpuscles are really abnormal, though their origin and mode of formation are doubtful. They occur either in the grey or white matter of the convolutions, scattered singly or in groups, as is seen in this drawing (Plate, Fig. VI.), or in layers on the surface closely aggregated, and bound down by the pia mater; they are round or oval in shape, highly colourable by carmine, which distinguishes them from colloid bodies, and frequently present a concentrically laminated appearance, more especially when they attain a very large size, as they occasionally do. An excellent example of this has recently come under my notice, the concentrically-marked body of large size being seen lying amongst the broken-down tissue from a superficial spot of softening on the surface of the corpus striatum. Amyloid bodies give with iodine a pale blue tint, which passes into violet on the addition of sulphuric acid, and when polarised exhibit black crosses, and the concentric rings already mentioned. It is still undecided whether these bodies are cells infiltrated with amyloid matter or concretions; their general uniformity of size, and their position and appearance, are more in favour of the former than of the latter hypothesis. The changes in the nerve-fibres, with which I have finally to deal, are less easy of demonstration than the lesions of the cells or neuroglia; they should be sought for in teased preparations of the fresh brain-tissue, the fibres being generally almost indistinguishable in transparent sections. Many of the morbid conditions

of this element of the nerve-centres have been incidentally mentioned when speaking of the lesions of the neuroglia on which they depend. Their normal condition may also be interfered with by hæmorrhages, exudations, and other external agencies acting on them.   In fresh-teased preparations we can frequently demonstrate the existence of globular and fusiform enlargements on the separated fibres, often proceeding to such an extent that the latter appear as little circular masses connected by fine fibre, or ending in flask-shaped or bulbous extremities, presenting a fine double contour, that portion contained within the inner border becoming lightly coloured by the staining fluid, while the outer ring remains unaffected.   What the patho-logical nature of this change is we are as yet unable to say.   Mr. Hamilton, in his recent investigations on myelitis, artificially induced, has shown that in the early stages of that condition oval swellings occur along the axis cylinder of the affected nerve-fibres, somewhat similar to the ampullations just mentioned; whether the latter, how-ever, have any connexion with the state he has observed, evidence is as yet wanting to show.   It is possible that the appearances may indicate a diminished consistence of the nervous texture which yields to the pressure of the covering glass used in the preparation of the specimens.   Rindfleisch mentions the occurrence in yellow softening of the brain of a somewhat similar change, which is doubtless due to diminished consistence from degenerative processes. Whatever be its nature this ampullation and bending of the nerve-fibres is interesting from a histological point of view, and worthy of further study.

In conclusion, I will merely observe that we have as yet but little clinical evidence whereby we can place our finger on any one case, and confidently assert that the symptoms it presents are indicative of any one of the pathological changes I have been describing; nay, indeed after death several morbid conditions of the nerve-elements may be found co-existent.   We can, as I formerly pointed out, from the broad features of a case speculate as to the probable condition of the vascular system, and, in a certain class, foretel—though I believe with much less certainty—the general change which may have taken place in the neuroglia, but beyond this we cannot mark any one lesion as peculiar to any one group of symptoms.   From the recent achievements in cerebral physiology I am inclined to believe that our work for the future must lie, not in the differentiation, but rather in the localisation of morbid changes in structure, as we may be able therefrom to connect

certain groups of symptoms with certain regions of the convolutions, and while doing so at the same time be able to draw some conclusions as to the psychical functions of such regions. Meanwhile the general results which have been attained in the pathological histology of insanity are interesting and valuable, as they prove beyond the possibility of a doubt that insanity is a disease, that its symptoms are the local expression of the disturbance of a physical organ, resulting finally, as in similar pathological processes occurring elsewhere, in structural lesions of tissue; hence its study should come solely within the domain of the physician, and therefore to him, and to him only, are the care and treatment of those affected with it now confided. Finally, while thus endeavouring to advance the doctrine of the corporeal nature of insanity, I trust I may not appear to some to be supporting the views of Materialism. Such be it far from me to do. The soul—that unfathomable, invisible, and undying part of man's nature—can surely never suffer from disease or decay; while bound to his organism by the tie of life, its manifestations closely intertwined with, and reached, as it were, through his mental faculties, may *appear*, with the latter, to become blighted, and withered, but once the "golden bowl is broken," and "the silver cord of life is cut," no longer trammelled by its physical union, the spiritual separates from the material, and the soul passes into that condition which it is destined by its Omnipotent Maker to occupy, until it will again be called to take its seat in the now undying body over which disease, decay, or death has no longer power; as Addison says:—

"The soul, secure in her existence, smiles
At the drawn dagger, and defies its point;
The stars shall fade away, the sun himself
Grow dim with age, and nature sink in years—
But thou shalt flourish in immortal youth,
Unhurt amid the war of elements,
The wreck of matter, and the crash of worlds."

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Transactions of the College of Physicians of Philadelphia. Third
Series. Volume the Second. Philadelphia: Lindsay & Blakis-
ton. 1876. 8vo, pp. 186.

WE have before us the second volume of the third series of " The
Transactions of the College of Physicians of Philadelphia." This
book, which is remarkably well brought out, contains some valuable
papers. In the commencement of the volume is a short memoir of
George W. Norris, M.D., and also one of John S. Parry, M.D., both
of Philadelphia. Although these papers are mainly of interest to
those who knew and associated with these undoubtedly able men,
yet they cannot be read by any member of their profession without
exciting a feeling of admiration towards men who spent their lives
in a zealous effort to advance the science of medicine and surgery.

In the former of these essays the writer, Dr. Wm. Hunt, speaks
in eulogistic terms of "Barton's bran dressing" in compound
fractures; this method, which consists in packing the injured limb
in a box splint filled with bran, appears worthy of more notice than
it has received in this country. The writer points out that with it
not only can equal pressure be kept up on all sides of the limb, but
the discharges are removed with ease and without disturbance,
while by the admixture of antiseptics with the bran, the wound
can be protected from the injurious influences of an hospital
atmosphere, and all fœtor effectually prevented.

The paper on a "Case of Empyema," read by Dr. James H. Hutch-
inson, is an additional piece of evidence of the value of the aspi-
rator in extracting fluid from the pleural cavity. The fact that the
subsequent introduction of a Chassagnac's drainage tube by means
of a curved needle round two ribs, and its retention within the
pleura for more than three months was not followed by any bad
effect, is highly calculated to give confidence to those who have
hitherto hesitated to employ this means of giving a permanent
exit to fluid within the pleural cavities.

The most remarkable paper in the book is that by Dr.
John Ashurst, on "Operative and Conservative Surgery of
the larger joints." In it are reported ten cases of excision of
the knee-joint, performed by this gentleman between the years

1870 and 1875, in all of which an excellent and useful limb was obtained without a single failure, and, in some instances, with unusual rapidity. In case IV., a boy aged nine years was discharged from the hospital cured, with "an excellent limb" four and a half months after the operation. In several other instances the patients were quite recovered within six months. All these cases were operated upon without the use of Esmarch's bandage, and the fact that in no one of them was there any of that consecutive hæmorrhage which has recently occurred in many cases of excision of large joints where it has been used, would tend to make it appear that valuable as this apparatus certainly is in many operations, yet it is not suited to cases of excision. Although, at the time he performed the operations recorded in this paper, Dr. Ashurst knew nothing of Esmarch's bandage, yet the following passage from his description of his method of operation supports the idea of those surgeons who think that it favours the occurrence of consecutive hæmorrhage especially in excisions of joints. He says:—

"I employ no tourniquet nor other means of interrupting the circulation during the operation, no large vessel is divided, and I believe it much safer to tie each small artery as it is cut than to run the risk (which is by no means only theoretical) of having consecutive hæmorrhage from vessels which under temporary compression have retracted, and which do not bleed until the patient becomes warm in bed."

This risk of consecutive hæmorrhage has certainly been found by some surgeons here to be " by no means only theoretical" in excisions of the knee, where Esmarch's bandage had been used. It does not appear that any antiseptic spray was used during the operations, and in one case of amputation it is stated that the limb was " dressed with laudanum in the usual way," from which it would seem that carbolic acid or salicylic acid is not usually used in Philadelphia, yet in many of the cases union is said to have taken place by adhesion.

On the whole this paper is not only a useful record of cases, but contains much valuable information as to the manner of operating, and the mode of after-treatment which led to an unusual degree of success.

There were many other very interesting papers read before the Society, among others that by Dr. Cohen " On the Therapeutic uses of Rarified and Compressed Air." The author of this paper expresses a conviction that in cases of emphysema, owing to dilatation

of the pneumonic vesicles consequent on asthma and chronic bronchitis, forced expirations into rarified air will prove beneficial. He gives in support a case in which after one month's treatment by this method the vital capacity of an asthmatic patient was "increased from 1,400 cubic centimeters to more than 1,600 cubic centimeters." So marked an improvement even in a single case is an important fact, but it seems probable that at the first measurement the vital capacity might have been temporarily diminished by a recent attack of asthma. This method of treatment of pulmonary affections, being thoroughly rational, deserves to be more fully tested.

We have no hesitation in saying that a few hours are well spent in reading this volume of "The Transactions of the College of Physicians of Philadelphia."

The Transactions of the Medical Society of the State of California during the years 1875–76. Sacramento. 1876. 8vo, pp. 168.

THESE Transactions are published in a volume of some 150 pages, which, if they do not contain much that is very new or original, prove, at least, that the State of California has within it a considerable number of energetic, thoughtful, laborious, medical men, whose common object is the advancement of medical science in that country.

There is an article on "The Treatment of Fever by Cold," by Dr. Dubois, in which the author describes his method of reducing the temperature of the body by means of a bed fitted with tubes for the circulation of water at the normal temperature of the body. This seems to imply that fever is to be treated as simply "*calor præter naturam.*" Although we cannot adopt this view, nor do we think that Dr. Dubois intends to imply so much, we do think that the plan suggested is capable of being applied for the purpose of reducing the temperature not only of the whole body as he proposes in cases of fever, but the running water quilt which he describes might be used for reducing the temperature in local inflammations in lieu of ice-bags, over which they would have the advantages of freedom from the danger of exposing the part to too great a degree of cold, and also of requiring less frequent attention if connected with a sufficiently large cistern.

Considering that this Society is yet in its infancy, the report of its Transactions for 1875 and 1876 is very creditable, and we may anticipate that its future reports will contain many valuable and original contributions to medical science.

Short Poems. Translated from the German by CHARLES A. CAMERON, M.D., F.R.C.S.I.; Hon. Mem. Royal Hibernian Academy of the Fine Arts. Edinburgh and London: William Blackwood and Sons. 1876. 8vo. Pp. 44.

WE have read this little book of poems with unmixed pleasure. The fact that it is the outcome of leisure moments in the busy life of one who is devoted to scientific pursuits invests it with peculiar interest; and the simple unaffected English of the translations shows that the author has caught the true spirit of the original poems. No doubt at times a line runs halting, and the full force of the German is not always expressed; but as a rule the rhythm is smooth, and the rendering close and accurate.

There are seven poems in the work—the "Christiana," of Matthias Clundius; "The Wanderer in the Saw-mill," of Kerner; "A Song of a Poor Man," by Ludwig Uhland; "The little Hydriote Boy," by William Müller; "Henry the Fowler," by Klopstock; "The Heath Inn," by Lenau; and "John Euler," by Seidl. Of all these, we like Uhland's pathetic "Lied" the best. We reproduce the last two stanzas in both German and English, and, in doing so, recommend this pretty little volume to the favourable notice of our readers:—

> " Noch leuchten Sonne, Mond und Stern
>   So liebevoll auch mir,
> Und wenn die Abendglocke hallt,
>   So red' ich, Herr, mit dir.
>
> Einst öffnet jedem Guten sich
>   Dein hoher Freudensaal,
> Dann komm auch ich im Feierkleid
>   Und setze mich ans Mahl."

> " Still shines the sun, the moon, the stars,
>   So fondly o'er me poured;
> And when the evening curfew tolls,
>   I speak with Thee, O Lord.
>
> " And when to each good man Thy halls
>   Of joy shall openèd be,
> Then shall I come in wedding garb,
>   And sit me down with Thee."

PART III.

HALF-YEARLY REPORTS.

REPORT ON PUBLIC HEALTH.[*]

By CHARLES A. CAMERON, M.D., Fellow and Professor of
Chemistry and Hygiene, Royal College of Surgeons, Ireland;
Medical Officer of Health and Analyst for Dublin, &c., &c.

GOOD AND BAD WATER FROM ONE WELL.—SPREAD OF TYPHOID
BY WATER.

IN general the water which is furnished by very deep wells or by
springs contains more mineral, and less organic, matter, than the
water found in the superficial wells of the same locality. The
reason of this difference appears to be the differential action of soils
upon soluble organic and inorganic substances. If solutions con-
taining ammonia, soluble albuminous substances, phosphoric acid,
and potash, be allowed to percolate through strata of clay, even of
no great depth, these substances will be wholly or almost com-
pletely removed from their solutions. If phosphoric acid be in
combination with ammonia both will be retained by the soil; but if
the phosphoric acid be in union with soda, then the latter will be
allowed to pass almost completely out of the soil, whilst the phos-
phoric acid will be retained therein. Again, if nitric acid and soda
in combination (sodic nitrate) be dissolved in water, and the solution
filtered through a thick bed of clay, the solution will pass through,
but slightly affected in composition. In the wells in which the
surface drainage water collects we find, very often, albuminous
matter, ammonia, phosphoric acid, &c., which the soil, from insuffi-
ciency of material, has not been able to separate from the water.
On the other hand, we rarely find these compounds present in the
water from arterial and other deep wells; but we generally find in
it large amounts of those salts which the soil appears to be incapa-
ble of separating from their solutions. These salts are chiefly calcic
sulphate, carbonate, chloride, and nitrate; magnesic carbonate,

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[*] The author of this Report will be glad to receive any books, pamphlets, or papers
relating to hygiene, dietetics &c. They may be forwarded through the agencies of this
Journal.

sulphate and chloride, and sodic nitrate and chloride. It often happens in Ireland that water from springs and deep wells contains from 70 to 150 grains per gallon of these earthy and alkaline salts, whilst it is almost absolutely free from organic matter and ammonia. If the salts include nitrates and chlorides in large proportion, many chemists consider them to be evidence of "previous sewage" contamination of the water—that is, they believe the sewage portion of water is deprived of its organic matter and ammonia during its percolation down through the soil; but that the common salt (which is always an ingredient of sewage), and the nitric acid (which is an ultimate product of the decomposition of nitrogenous organic matter) pass through the soil, and are found in the deep drainage water. There are waters from deep wells which contain large amounts of solids, but do not include much nitric acid or common salt, whilst in some the common salt is abundant and the nitrates absent. Great stress is laid upon the presence of common salt; but we have no doubt that in many well-waters in which it is contained it by no means indicates "previous sewage impurity." The sandstone and other sedimentary deposits, which constitute the prevailing rock formations in most countries, having been formed upon the bed of the sea, it is not unreasonable that the water percolating throughout them should meet with and dissolve common salt, and, occasionally, in abundance.

Having now explained the causes which produce differences in the composition of the deep wells of a district, as compared with the shallow wells of the same localities, we desire to record a singular instance of two kinds of drainage waters, occurring in the same well and at the same time. We found the composition of two specimens of water from Glenfarne Hall, county of Fermanagh, to be as follows. An imperial gallon (70,000 grains) of each contained—

	No. 1. Grains.	No. 2. Grains.
Total solid matters, - -	29·012.	47·4000
Including—		
Albuminoid nitrogen, - -	0·025	0·0060
Ammonia, - - - -	0·010	0·0003
Nitrous acid, - -	- large amount	none
Nitric acid, - - -	- small quantity	trace
Calcic sulphate, - -	- 6·000	6·5000
Chlorine, - - - -	2·137	1· 795

No. 1 water was taken from a well by dipping a vessel into it, but not to a greater depth than was necessary to fill the vessel. No. 2 water was taken from a tap in the house, but which was supplied from the well which afforded No. 1 specimen. Here was an anomaly—good and bad water taken simultaneously out of the same well! Could there be a mistake in selecting these samples of water for examination? Could an error have been made in their analysis? These questions naturally suggested themselves, and to answer them satisfactorily fresh specimens were obtained, under the immediate directions of Mr. A. Loftus Tottenham, D.L., in whose residence the well exists. These new specimens were carefully examined, and found to differ widely in composition—one being very hard (from large contents of solid ingredients), and free from organic impurities, the other comparatively soft, but containing much organic matter—as shown by the large proportion of albuminoid nitrogen which it contains.

The only explanation which we can offer to account for good water and bad being simultaneously present in this well is that it is supplied with drainages from two sources. The well is very deep, and the pipe which conveys water from it to the tap (No. 2) extends to within a few inches of its bottom. Probably the water which enters the lower part of the well, through its side and bottom, is derived from springs, or at any rate it is water which had percolated throughout a considerable quantity of clay, and had thereby been deprived of any organic matter which it might originally have contained. On the other hand, the drainage of the surface of the surrounding soil must have in part made its way into the well, through the sides, but near its mouth. As this drainage would undergo but little filtration, it would probably be contaminated with organic matter, as surface drainage so generally is. It was this surface drainage which undoubtedly furnished the impure water (No. 1) taken from the upper part of the contents of the well. That it is possible, and even easy, to have two kinds of water in a long narrow vessel we have satisfied ourselves by the results of an experiment made in the laboratory. The useful lessons to be derived from these curious facts are—1st. That when the water of a well is to be examined, care should be taken to obtain a specimen representing the average composition of the *whole contents* of the well. 2nd. That the discrepancies (so often observed) between the analyses made by different chemists of the water from identical sources—*i.e.*, wells, may be explained by supposing that the samples

were really different, as we have seen was the case in one authentic instance.

A case in which pipe-water, originally perfectly pure, became locally highly contaminated, and with fatal results, came recently under our notice. In the suburban dwelling of a well-known and popular clergyman, no fewer than seven cases of typhoid fever occurred within a month. We were asked to make a sanitary inspection of the house, which we did, with the following result:— We found that the sewerage arrangements were good, but we traced the overflow-pipe from a water-closet cistern to a second cistern, which was supposed to furnish water only for a bath. We found, however, that the water from this cistern was often used to supply the water-carafes in the bed-rooms. In this case pure water, in the first instance, entered the cistern which supplied liquid to the water-closet. When water was let down into the pan of the water-closet a certain amount of foul air ascended into the water, and to a great extent dissolved therein. As the overflow-pipe from the cistern passed into the soil-pipe, the foul gases in the latter must occasionally have passed up into the space over the water-cistern, and have become absorbed by the water. In this case the water was highly and even obviously polluted. This was one of those sudden and localised outbreaks of typhoid which, according to the late Professor Parkes, are occasioned by contaminated water. Had the inmates of the house not used this polluted water, the fever would probably have been confined to the first individual who contracted it, and whose dejections, without doubt, contaminated the water in the cistern in the way which we have explained.

It is a fact, but one not generally known, that air is often sucked into pipes through which high-pressure water is flowing. If in such a pipe there are small holes (as sometimes happens), the water will pass out through them in the form of jets, which will diminish in size if a tap be opened close to them. Now, when by the opening of a tap near where there is a hole in a water-pipe, the jet from the latter is diminished in size and force, then air is liable to be sucked into the pipe. As leaky taps may be regarded as equal to holes, it is not safe to have taps over sinks or other places from which foul air is likely to issue. It is for this reason that it is improper to flush the pan of a water-closet by discharging water directly into it from a tap, as we know is often done; if whilst such a tap was open the pressure of water was diminished in the main, an insuction of foul air into the pipe would certainly occur.

In the Seventh Annual Report of the Massachusetts Board of Health, January, 1876, p. 467, there is recorded a remarkable case of an outbreak of enteric disease, caused by the use of impure ice. The complaint broke out amongst the inmates of a hotel at Rye-Beach, New Hampshire. A search for the *origo mali* discovered it in the ice, which, being summer-time, was in ample request amongst the sojourners in the hotel. The ice was found to contain much decomposing organic matter, and it was noticed that it possessed a somewhat offensive odour. The ice and the water of the *pond,* from which it had been procured, were analysed by Professor W. R. Nichols, who found in both excessive amounts of organic impurities. The hotel was capable of accommodating 300 persons; but only 26 of the guests appear to have been decidedly affected by the ice.

SMALL-POX AND VACCINATION.

We trust that we may be mistaken, but we predict that small-pox will become epidemic in Ireland in 1877. This loathsome disease has been slowly but steadily spreading throughout many parts of England during the last two or three months, and if it become rife in Liverpool we may expect its early arrival in Dublin. The enemy may not come, but, should it, let us be prepared to meet it. The history of epidemics, if it teaches us anything, certainly proves that the zymotic exanthemata flourish most where filth abounds. The lesser virulence which plagues in modern European cities exhibit, as compared with their ravages in mediæval towns, is only to be explained by the purer water supplies, the better ventilation, the superior sewerage systems, the more complete removal of refuse, and the discontinuance of intra-mural interments, which distinguish the former. Although it must be admitted that the urban populations are now placed under far better hygienic conditions than their predecessors of the last and previous centuries were, yet vast is the sanitary work which remains unaccomplished in most of our towns, and more especially in those of Ireland. The Commissioners recently appointed to inquire into the conditions of these towns, in relation to their local government, have elicited startling revelations in reference to the all but complete neglect of sanitary measures in the majority of them. In many important towns the water used by the inhabitants is merely diluted sewage, and still more generally is there a want of out-offices for the deposition of excreta and refuse. We, ourselves, found that even

in the fashionable seaside town of Bray, the dwellings of the humbler classes were, with few exceptions, unsewered, and unprovided with privies or ash-pits.

It cannot be denied that the habits of the lower classes in Ireland are opposed to neatness and cleanliness. It is this indisposition to keep themselves and their dwellings clean which opposes so great a barrier to sanitary reforms. There cannot be a policeman or a sanitary sub-officer watching day and night every lodging-house or mean dwelling in order to enforce cleanliness upon its inmates; still much more might be done to further this object than is now attempted by municipal governing bodies. They have the power to compel the owners of houses to provide them with proper and decent appliances for the reception and storage of the effete matters produced therein; and for the removal in good time, and in the least objectionable way, of these matters. These and other obvious sanitary measures are urgently called for in almost every Irish town, and if they are not attended to by the local authorities they should be compelled to do their duty by the intervention of a higher power. Now that an outbreak of small-pox in Ireland is even more than a possibility, the local authorities should shake off their lethargy, and hasten on the good work of sanitary reform. The Public Health Committee of the Corporation of Dublin are considering the best means of preventing the spread of small-pox, should it be introduced into Dublin. They have decided that the clothes, bed-clothes, and bedding of all persons who may contract the disease shall be destroyed by fire; and, in the case of poor persons, compensation given to the owners or their representatives. The Committee should, however, enforce general cleanliness, the isolation of the affected, the prompt removal of the sick to hospital; they should provide an hospital for the reception of convalescents, and should prosecute those who, before their power to communicate the disease had ceased, freely mixed with unaffected persons. All this the law enables them to do, and it is to be hoped that no mistaken and petty economy will prevent these and other useful sanitary precautions from being adopted.

There is one precaution against small-pox which municipal authorities cannot enforce, and that is re-vaccination. In former Reports we adduced what to our mind appeared to be conclusive evidence in favour of this operation. Whether it be that re-vaccination has proved beneficial because the primary operation had not been successful, or because, though successful, its prophylactic power

had died out in the lapse of time, is of no practical importance. Statistics show that the re-vaccinated are, as a rule, less liable to contract small-pox, and suffer less from the disease when they do catch it, than those who had been vaccinated in infancy. On the other hand, those who have been vaccinated stand in even a more favourable relation to those who have never been vaccinated in any way. This is particularly the case with children. Thus, the English Registrar-General, in his report for the week ended 2nd December, 1876, states that up to the 25th November in the metropolitan district only one child out of 317,081 vaccinated children had died from small-pox, whilst during the same period 35 among 31,360 un-vaccinated children had perished from that disease. On inquiry, too, it was discovered that the vaccinated child who died of small-pox had shown symptoms of the disease eight days after being vaccinated; so that it had in all probability contracted the disease before it had been vaccinated. Mr. Marson, one of the most eminent authorities on the etiology of small-pox, states that it makes its appearance on the skin *fourteen days after the introduction of the poison into the system.* It is also asserted on good authority that vaccination does not become protective until the twentieth day, when the scab falls off.

Mr. John Prior Purvis, in a Report on Statistics of Vaccination,[*] states that the operation is often insufficiently performed by private practitioners. His conclusions in relation to this and some other points are as follows:—

" *Firstly*, it would seem desirable that a clause should be inserted in the next amendment to the Vaccination Acts, legally defining the number of vesicles necessary to constitute vaccination, and fixing four as the minimum number that a medical man should at all events *attempt* to produce.

" *Secondly*, that re-vaccination at puberty, say at about fifteen years of age, should be made as compulsory as the present primary vaccination is.

" Were it possible to enforce by legal enactment or otherwise these two all-important points, we might hope that the next wave of epidemic small-pox would find a population all but insusceptible of its influence.

" Lastly, there are four points in my practice to which I wish to direct especial attention.

" 1. The picking up the lymph from the opened vesicles of the vaccinifer, by means of a capillary tube.

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[*] Statistics of Vaccination at the Greenwich Vaccination Station. A Report presented to the Board of Guardians, 1st Feb. 1876.

"2. The registration of the character of the vesicle on the eighth day, by means of a letter.

"3. The keeping of charged tubes in little bottles in a case, instead of on slips of card.

"4. The mixing of an equal quantity of glycerine with lymph for re-vaccinations."

Mr. Francis Vacher, in his valuable Annual Report (for 1875) on the Sanitary Condition of Birkenhead and Claghton-cum-Grange, states that by prompt sanitary measures two local outbreaks of small-pox in those districts were prevented from spreading. The means adopted comprised the destruction of clothing and bedding likely to be infected, thorough disinfection of the houses in which the disease had appeared, isolation of the sick, and a house-to-house inspection by the public vaccinator. Such prompt and energetic measures deserved that success which happily attended them.

POISONOUS EFFLUVIA FROM FACTORIES.

There is no country in the world in which there are so many industries carried on as in the British Islands. In Ireland, perhaps, unfortunately, those industries are found in their *minima*. In the neighbourhood of Glasgow and Newcastle-upon-Tyne, in Lancashire, Warwickshire, Staffordshire, and Yorkshire, they attain their *maxima*. The workers in these industries are subject to diseases which have their origin in the peculiarities of their occupations. These are the greatest and most numerous sufferers from the factories. Less numerous are those who, not deriving any immediate benefit from factories, are injured in health or rendered uncomfortable by being near neighbours of factories, from which noxious gases and vapours are evolved. It is chiefly in Lancashire and in the neighbourhood of Swansea, and Newcastle-upon-Tyne, that factories are the greatest nuisances. In these districts the manufacture of soda-ash, the' smelting of copper and lead, and many other chemical industries, are chiefly carried on. They give rise to the evolution of immense quantities of hydrochloric and sulphurous acids, vapours of lead and arsenic, and enormous volumes of black smoke, with its concomitant of deadly carbonic oxide gas. The damage done by the chemical works of Lancashire is so great, especially to the agriculture and flora of that great county, that at present strenuous efforts are being made to lessen it by means of legislative interference.

Artificial manure manufactures have now been established in a

great many districts. There are four of them in Dublin. In them sulphuric acid is prepared upon a large scale, and used in mixing with bones and mineral phosphates of various kinds. During this admixture several gases are disengaged, notably fluoric acid and fluoride of silicon. They are, if inhaled, irritative and poisonous, but more especially the former. As the iron pyrites, which is used in making sulphuric acid, invariably contains arsenic, it may be that the emanations from the manure works contain this poison in a gaseous form. This is asserted to be the case by Dr. James Adams.[a] According to this gentleman, a Mr. Frazer met with his death by inhaling arsenical vapours which emanated, together with other matters, in large volumes, from a manure works. The gentleman was proceeding by train from Bunchrew to Inverness, and happened to remain for a short time at a way-side station, during which he was exposed to " a volume of offensively-smelling vapour, that was suddenly disengaged from a bone manure work which practically forms part of the station." He commenced to vomit and continued ill until his arrival at Inverness, where he died three days after (on the 21st Feb., 1876).

This case has excited much attention, and has been discussed at great length in the public journals, and especially in *The Chemical News*. It appears that Mr. Frazer had commenced proceedings shortly before his death to compel the owners of the manure works to close them, alleging that the noxious matters evolved from them had injured his health. The proceedings were carried on by his trustees, and resulted in the closing of the works. The judge who tried the case stated that he saw no reason to doubt the assertion that the exhalations from the works originated Mr. Frazer's illness. At the trial, and during the discussion which followed it, there were, as usual, opposite opinions expressed by the experts. Some of them asserted that arsenic could not exist in the vaporous matters evolved from the works, whilst others maintained the contrary opinion.

The presence of arsenic, under such circumstances, is not unlikely, seeing that sulphuric acid contains from 0.2 to 0.5 per cent. of that substance. As considerable heat is disengaged during the action of the acid upon the raw phosphates, arsenic is doubtless often volatised, though it is almost certain to be condensed in the flues. It seems only reasonable that the manufacturers of artificial manures should be compelled to adopt every precaution likely to prevent

[a] On the Presence of Arsenic in the Vapours of Bone Manure. By James Adams, M.D. 1876.

any of the dangerous volatile products of the action of sulphuric acid upon animal matter or mineral phosphates from passing into the atmosphere; it is sufficiently easy to condense them by the application of water in fine jets.

It does not appear to be generally known—at least we have never seen the matter referred to in works on hygeine—that glass bottlè-makers often use arsenious acid in large quantity. When the melted glass requires to be oxidised, white arsenic (arsenious acid) is occasionally introduced into it, and to the extent of perhaps a stone weight at a time. The greater part of the arsenic is immediately volatised, and must prove a very poisonous addition to the atmosphere of the immediate locality.

INFANTILE MORTALITY AMONGST THE CHILDREN OF FACTORY OPERATIVES.

Dr. C. D. Purdon is doing useful and humane work in directing public attention to the excessive mortality which exists amongst the children of the factory operatives in Belfast and its neighbourhood, and in suggesting means whereby it may be lessened. Dr. Purdon has collected statistics of the births and deaths amongst the labouring, artisan, and well-to-do classes. He states in a pamphlet,* recently published, that of the children of the mercantile and professional classes, and of the gentry, 15 per cent. die during the first two years and a half of their existence, whilst among the children of the artisans and labourers, 20 per cent. die during the same period. Amongst the children of the factory operatives the mortality is still greater, amounting to 35 per cent. of them dying before they attain the age of two and a half years. There is even a differential mortality amongst the children of the mill operatives. It appears that women are chiefly employed in the preparing, reeling, and weaving departments of the linen mills. Large numbers of them are married, and of these a considerable proportion are obliged to work for the maintenance of their families, owing to the carelessness or inability of their husbands. When these women have infants they are obliged to consign them, during working hours, to the "care"(?) of old women. It is amongst these infants that Dr. Purdon found the highest mortality; he describes it as " enormous."

* Infantile Mortality amongst the Children of Factory Workers, as compared with other Classes of the Community. By C. D. Purdon, M.B., F.R.S.I.; Certifying Surgeon to the Belfast Factory District. Belfast : Adair, Arthur-street. 1876.

Speaking of flax mill workers, Dr. Purdon states that the deaths amongst them exceed the births. As Belfast is increasing rapidly in population, the increase is no doubt chiefly due to immigration.

The use of " soothing syrups "[a] (*i.e.*, preparations of opium) is, we learn, enormous, and increasing in Belfast; no doubt they keep babies very quiet, and often relieve their parents from any trouble in relation to them, except the cost of burial, which, however, is generally provided by Tontine Societies or the Board of Guardians. As one remedy for the evils which he has so forcibly pointed out, Dr. Purdon suggests the establishment of *crèches*, or day-nurseries. The great advantages of these institutions for the care of helpless infants has been forcibly pointed out by Mr. Baker in his suggestive paper on " Infant Mortality."

In the following table (part of a very elaborate one) are shown the average annual deaths of children amongst various classes during the year 1874:—

	Number that were born in 1874	Number that died in 1874 under 1 year	Number that died in 1874 under 2½ years	Average of those that died under 1 year	Average of those that died under 2½ years	Total Average
Flaxdressers, - - - -	405	60	30	14·8	7·4	22·2
Millworkers, - - - -	16[b]	57[b]	10	356·4	62·5	418·8
Tenters, - - - -	57	12	4	21·1	7·0	28·1
Bundlers, - - - -	24	4	2	16·6	8·3	25·0
Overlookers, - - - -	47	10	5	21·3	10·6	31·9
Dressers and Beamers, - -	19	6	1	31·6	5·3	36·9
Oilers, - - - -	6	1	—	16·6	—	16·6
Weavers, - - - -	25	16	6	64·0	24·0	88·0
Warpers, - - - -	1	—	—	—	—	—
Winders, - - - -	—	2	—	—	—	—
Cloth Passers, - - -	3	5	1	166·6	33·3	200·0
Spinners, - - - -	—	—	—	—	—	—
	603	173	59	28·7	9·8	38·5
Number born in the Artisan and Labouring Class in 1874, -	6,725	995	370	14·8	5·5	20·3
Number born in the Gentry, Professional, and Mercantile Class in 1874, - - -	114	12	6	10·5	5·3	15·8

[a] Some years ago we examined a specimen of the well-advertised " Winslow's Soothing Syrup," and found it to contain nearly one grain of morphia per oz. A teaspoonful would, therefore, contain an excessive dose of morphia for an infant.

[b] These figures appear to be incorrect or transposed.—[C. A. C.]

The gentry, and the mercantile and commercial classes in Belfast, must be ardent disciples of Dr. Malthus, seeing that in a population of nearly 200,000 only 114 children, belonging to these sections of the community, were born in 1874.

" DEATH IN THE POT."

It is now many years since Mr. Accum startled the British public by the production of his book, entitled " Death in the Pot." The author treated upon the subject of the adulteration of these articles of which a large proportion were cooked in pots, and which contained more or less of poisonous matters. It is bad enough to have poison put occasionally into our pots, but what is that compared with having the vessels themselves actually in part composed of poison! Such, however, we are informed by a trusty and well-known chemist, is often the case.

At a meeting of the Society of Public Analysts of Great Britain and Ireland, held at Glasgow last September, Mr. R. R. Tatlock, F.R.S.E., read a paper on " Enamelled Cooking Vessels." He showed that the enamels used varied considerably in composition. Some of them contained large quantities of lead and decided amounts of arsenic, whilst in others these poisonous substances were present in minute proportions, if at all. In the following table is shown the composition of the white enamels found on three cooking-pots of cast-iron:—

Composition of Enamels.

	1. Per Cent.	2. Per Cent.	3. Per Cent.
Silica,	61·09	42·40	42·00
Alumina,	8·00	2·88	6·06
Oxide of Iron,	1·10	2·04	4·04
Lime,	3·02	·16	·78
Magnesia,	·28	·10	·21
Oxide of Lead,	Absent	25·89	18·48
Potash,	5·61	7·99	6·46
Soda,	20·67	14·67	19·25
Phosphoric Acid,	Trace	Trace	Trace
Arsenious Acid,	·02	·42	1·02
Carbonic Acid,	·30	Absent	Absent
Boracic Acid,	Absent	3·45	1·70
	100·00	100·00	100·00
Bases,	38·68	53·73	55·28

Mr. Tatlock found that a solution of citric acid, containing only one per cent. of the acid, dissolved arsenic and lead when boiled in No. 3 pot.   Gooseberry jam and a solution of salt boiled in the vessel took up lead, but the latter did not dissolve the arsenic, nor could the presence of that poison be clearly demonstrated in the jam.   It is evident that the employment of such vessels may be prejudicial to health; therefore the use of poisonous enamels should be rendered illegal by statute.   Mr. Tatlock states there was nothing peculiar in the *appearance* of the poisonous enamel by which it could be discriminated from the non-poisonous kind.   He found, however, that a weak solution of citric acid, boiled on the lead enamel, renders it dull, whilst under similar treatment the non-poisonous enamel retains its glassy appearance.

It is probable that slow lead-poisoning—unsuspected or believed to be disease of natural origin—is occasionally produced by the use of acid food prepared in vessels such as those described by Mr. Tatlock.

THE TRANSMISSIBILITY OF SYPHILIS BY THE MILK.

To test the question—Is syphilis transmissible by the milk? R. Voss injected the milk of a syphilitic woman into three prostitutes.   The woman was suffering from a papular syphilide, with condylomata on the genitals and anus; the breasts were, however, quite free from disease. The milk was obtained by expression, and a Pravaz's syringeful was injected into each prostitute.   The first being syphilitic, the inoculation was naturally without result.   The second suffered from a urethritis, and she remained unaffected.   The third, sixteen years old, who had never been affected with syphilis, had come into hospital on 16th September, for a urethritis, and was subjected to the injection of the milk on the 27th.   The injection produced a large inflamed swelling, similar to what occurred in the first patient, and this gradually diminished and disappeared by 24th October.   On 3rd November—that is, forty days after inoculation—a papular eruption showed itself round the seat of the injection; and on the 8th, the whole of the body was covered by a papulo-macular syphilide.   The glands were also generally affected.   These symptoms disappeared under inunction.   R. Voss concludes, from this fact, that the milk of a syphilitic individual is quite as likely a means of propagating syphilis as the blood.—*Petersb. Med. Wochensch.,* 1876, No. 23; *and Centralbl.,* No. 44, 1876.

<div align="right">J. M. F.</div>

PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

SAMUEL GORDON, M.B., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, December 6, 1876.

DR. FITZPATRICK in the Chair.

Salicylic Acid in Acute Rheumatism. By JOHN WILLIAM MOORE, M.D., Dubl.; Fellow and Censor, K.Q.C.P.; Physician to the Meath Hospital and County Dublin Infirmary.

"AMONG the results obtained from the internal use of salicylic acid, its effects not only as an antipyretic but almost—if the statements already made be supported by further observations—as a curative, in rheumatic fever, are most noteworthy." These words, which formed the opening sentence of an excellent summary of the views of Stricker, Riess, Broadbent, Maclagan, and others on the use of salicylic acid and salicin in acute rheumatism,[a] suggested to me the compilation—for the Medical Society of the College of Physicians—of this account of the results obtained at the Meath Hospital in the treatment of acute rheumatism by salicylic acid or its salts. I am no enthusiastic advocate of, or believer in, the specific virtues of the medicine, which has now been tried by most of our hospital physicians in Dublin. Should this paper elicit their opinion of the value of salicylic acid in acute rheumatism, my object in writing it will be fully attained. Meanwhile my endeavour has been to make the present communication simply a faithful clinical record of this special treatment in the few cases which opportunity has as yet brought under my observation.

The exceptionally warm and dry summer of this year reduced the prevalence of rheumatic fever to a minimum. But a great fall of temperature early in September, followed by a persistent rainfall from the 22nd of that month to October 20th, soon produced a rapid increase in the number of cases of this most painful affection. Thus the opportunities for clinical observation, which had been singularly few for many months, became frequent, and were made use of to the best of my ability.

[a] London Medical Record. June 15, 1876. Page 241.

Six cases of acute rheumatism, of varying degrees of severity, were treated in the wards of the Meath Hospital, over which I have charge, between September 26th and November 20th of the present year.   One patient suffered a relapse, after having been a fortnight convalescent.   The first four cases were carefully observed by my former clinical clerk, Mr. James A. MacCullagh, to whom I am indebted for many of the notes, and by practising pupils Mr. Sherrard, Mr. Atkinson, and Mr. W. Miley. The last two cases were under the charge of my present clinical clerk, Mr. Blacker Powell, and of practising pupils Mr. H. Smith and J. Conolly.

CASE I.—*Acute Rheumatism, with commencing Pericarditis ; Salicylic Acid in 5-grain doses every hour; Apyrexial in three days; total amount of Salicylic Acid taken = 180 grains; Relapse; Treatment resumed on third day; Apyrexial in three days.*

Bernard C., aged fourteen, a porter in a wine-shop, got a heavy wetting on Saturday, September 16th, 1876.   He had been previously healthy and had never suffered from rheumatism.   On Thursday, September 21st, his right ankle and foot swelled, with severe pain.   There were "achings" in the other joints.   A shivering fit occurred on this day, and he took to his bed.   He had neither headache nor sick stomach, but his appetite was completely lost.   Joint after joint became affected, until at last he sought admission to hospital on Tuesday, September 26th, the sixth day of his illness.

At this time his pulse was 98 ; respirations 26 ; and temperature 101°. I saw him at 6 p.m., when the temperature had risen to 102·3°.   His face bore an aspect of intense suffering.   The following joints were swollen, tense, red, and exquisitely painful—viz., the right knee and ankle, the left wrist and elbow, the left knee and ankle, and the right wrist.   The right elbow was slightly affected and the shoulder-joints were painful, but not swollen.   A marked visible pulsation in the carotid arteries led to a close examination of the heart.   There was pain in the epigastrium, which was increased to an intolerable degree on even the gentlest pressure. A single systolic murmur was heard at the base of the heart, whence it was carried into the vessels.   In addition to this, a well-marked "to-and-fro" friction rub was audible also at the base.

Diagnosis.—Acute rheumatism, with commencing pericarditis, and probably endocarditis.

Treatment.—Three leeches were applied to the precordial region, and the bites were allowed to bleed into a large linseed meal poultice. Salicylic acid was given in 10-grain doses every third hour, 10 minims of tincture of digitalis being added to each dose.   A comparatively quiet night followed.

September 27th (seventh day).—Pulse 90 ; respirations 26 ; temperature

100·2°. The area of precordial dulness is increased. The systolic murmur is now heard at both base and apex. It is not so distinct as it was yesterday. The friction sound is also lessened in intensity. The salicylic acid was now given in 5-grain doses every hour, in emulsion with glycerine and water.

September 28th (eighth day).—Pulse 78; respirations 26; temperature 98·8°. He has been comparatively free from pain since last night, and slept well. His tongue is cleaning. The evidences of pericardial mischief are much less marked. The apex murmur is better heard, but that at the base is less audible. The salicylic acid was stopped when he had taken 180 grains. After this day there was not an untoward symptom, and he was kept in hospital simply to watch the systolic murmurs at the base and apex of the heart. To test their nature, he was put on citrate of iron and quinine on October 5th, when he sat up for the first time. The murmurs were very variable; but on the whole they were fading away when he left hospital on Saturday, October 14th, for the purpose of going to the Convalescent Home, Stillorgan. Unfortunately the day was bleak and cold, and, as the event proved, he caught cold, which led to a relapse.

Bernard C. came back from the Convalescent Home, Stillorgan, on Monday, October 16th. He had felt a chill when going to bed on Saturday, October 14th, and had headache, shivering, and pain in his joints next morning. On admission, his temperature was 100·8°, rising in the evening to 102°; pulse 102; respirations 28; he complained of great pain in all his joints, but his left wrist was the only one swollen. The murmur at the apex of his heart, which was but faintly heard when he left hospital on Saturday, was now much more distinct, otherwise the heart was healthy. He was at once put on 5-grain doses of salicylic acid every hour.

Next morning his temperature had fallen to 99·4°, being in the evening 100°; pulse, 96; respirations 25; he had slept fairly, and was suffering much less pain; the swelling had pretty nearly left his wrist; the apex murmur was still distinct.

On October 18th, his temperature was 98·6°, in the evening 98·8°; pulse 86; respirations 20; he had slept very well; had no pain whatever; the swelling in his wrist had completely subsided, and the murmur was not so distinct as on the previous day. The salicylic acid was stopped when he had taken 120 grains.

On October 20th, his morning temperature being 99°, he was ordered 5-grain doses of salicylic acid every three hours; in the evening his temperature had fallen to 98·5°, and the salicylic acid was discontinued; the murmur was almost quite gone; he was perfectly free from pain. He continued to improve from this; and, on October 26th, he went out quite well, the murmur being scarcely perceptible.· He has since then called at the hospital on two or three occasions to report himself, so that I am justified in saying that he continues well.

Record of Pulse, Respirations, and Temperature.

Day of Disease	MORNING			EVENING			Remarks
	Pulse	Resp.	Temp.	Pulse	Resp.	Temp.	
6	98	26	101·0	104	30	101·9	Temp. 102·3° at 6 p.m.—Leeches to precordium. } Salicylic acid 180 grains in all.
7	90	26	100·2	96	30	101·3	
8	78	26	98·8	70	22	98·8	
9	64	20	98·2	66	20	98·6	
10	70	22	98·4	66	22	98·4	
11	64	22	98·4	66	22	98·4	
12	62	20	98·2	60	22	98·0	
13	62	20	98·2	60	20	98·2	
14	70	20	98·2	66	20	98·4	
15	62	20	98·6	60	20	98·6	Citrate of iron and quinine.
16	62	20	98·7	68	20	98·4	
17	66	20	98·0	66	20	98·4	
18	64	20	98·2	62	19	98·4	
19	66	20	98·2	60	20	98·6	
20	66	20	98·6	80	20	98·5	Roast mutton for dinner to-day.
21	80	20	99·0	78	20	98·5	
22	70	20	98·8	74	20	98·8	

Relapse.

Day of Disease	MORNING			EVENING			Remarks
	Pulse	Resp.	Temp.	Pulse	Resp.	Temp.	
III.	102	28	100·8	122	26	102·0	Twenty-sixth day of first attack. Salicylic acid 60 grains.
IV.	96	25	99·5	100	24	100·0	Salicylic acid 60 grains.
V.	96	20	98·6	74	19	98·9	
VI.	80	18	98·3	76	18	98·6	
VII.	82	20	99·1	84	19	98·6	„ „ 60 „
VIII.	82	20	98·3	80	18	98·4	
IX.	84	20	98·4	88	20	98·6	
X.	88	20	98·4	100	20	98·9	
XI.	84	20	97·9	80	18	98·2	
XII.	80	18	98·0	?	?	?	} Evening observations not taken.
XIII.	78	18	98·0	?	?	?	

CASE II.—*Acute Polyarthritis Rheumatica supervening on subacute Articular Rheumatism; Treatment by Salicylic Acid from eighth day; complete relief and freedom from fever after 150 grains had been taken.*

Catherine B., aged sixty, was admitted to the Meath Hospital on the 6th of October suffering from acute rheumatism. In the beginning of May, 1876, she had got a severe wetting, and a couple of days after a pain came in her back; this pain shifted about from one joint of her body to another, and she had to lie up for a couple of weeks. She got pretty well after a while, but has had attacks off and on ever since, and, though she could walk about, has been unable to work. On Saturday, September 30th, a great pain came in her feet, and on Sunday morning

her feet and knees were very much swollen, and the pain was so great she could not turn in bed. She had no shivering or sickness of stomach, but suffered from a slight headache, and had altogether lost her appetite. On admission, on the eighth day of her illness, her temperature was 102·6°, rising in the evening to 103°; pulse 108; respirations 20. The following joints were very much swollen—left knee, left elbow, right elbow, left wrist, right wrist, right knee, left ankle, right ankle. She complained of her shoulders being stiff, but they were not swollen; the heart was perfectly healthy. The affected joints were enveloped in cotton wool; she was ordered beef-tea and milk, and half an ounce of the following mixture every hour and a half. ℞. Acid. salicylici, gr. 80; glycerini ℥i.; aquæ fervidæ ad ℥viii. Next morning her temperature was 101°, falling in the evening to 99·9°; pulse 106; respirations 20. She had slept fairly. The swelling in the joints had greatly subsided, and she said she was suffering scarcely any pain. Her bowels had not acted.

The following day her morning temperature was 98·6°; evening, 99·2°; pulse 86; respirations 22. She had slept very well. The swelling had altogether left her joints. She was entirely free from pain, and had sat up in bed to eat her breakfast. Her bowels had not acted. The salicylic acid was discontinued. She had taken 150 grains.

On October 9th, her temperature was 98·2°; pulse 85; respirations, 22. She had slept all night, and felt no pain. Her bowels had acted twice.

On October 13th, as her temperature was 98·9°, and she complained of some slight soreness in her left shoulder, she was ordered 5-grain doses of salicylic acid every third hour. Next morning the soreness had left her shoulder, her temperature was 98°, and remained so till she went home on October 16th. The salicylic acid was again discontinued on the morning of October 14th, when she had taken an additional 50 grains.

Record of Pulse, Respirations, and Temperature.

Day of Disease	Morning			Evening			Remarks
	Pulse	Resp.	Temp.	Pulse	Resp.	Temp.	
8	108	20	102·6°	112	22	103·0°	Salicylic acid 80 grains.
9	106	20	101·0	100	22	99·9	„ „ 70 „
10	86	22	98·6	96	22	99·2	
11	84	22	98·4	80	22	99·2	Mist. sennæ co. ℥j.
12	76	22	98·4	78	22	99·2	
13	80	20	98·8	78	20	99·4	
14	80	22	99·2	78	20	99·2	
15	80	20	98·8	76	19	99·4	Salicylic acid 60 grains.
16	80	20	98·6	78	18	98·6	
17	72	20	98·1	76	19	98·0	
18	80	20	98·2	—	—	—	Left hospital.

CASE III.—*Polyarthritis Rheumatica Acuta ; Treatment by Salicylic Acid from fourteenth day, in 5-grain doses every hour; total quantity used,* 250 *grains ; apyrexial in seven days.*

Julia R., aged thirty-three, was admitted to the Meath Hospital on October 7th, suffering from acute rheumatism.

On Saturday, September 22nd she got wet feet, and next day headache and a shivering of cold set in. She also lost her appetite, and "lumps came in her neck." She was not bad enough to take to bed, and the following Thursday felt nearly quite well. On Friday, however, her feet swelled, and on Saturday her knees became affected. Her joints were dreadfully painful. She got worse from day to day, and the swelling went from one joint to another. She never had rheumatism before, but used to have twinges of pain, off and on, in the joints during the winter. On admission, on the evening of October 7th, her temperature was 102·6°; pulse 108; respirations 24. The following joints were very much swollen, red, and painful—the left knee, right elbow, left ankle, right knee, right wrist, left elbow, left wrist ; the right shoulder was also much swollen. The right ankle and left shoulder were not affected. Her heart was healthy. On admission, she seemed to be in great pain, could not move any of the affected joints, and would not allow them to be touched. Her joints were enveloped in cotton wool, and she was ordered half an ounce of the following mixture every hour:—℞. Acidi salicylici, gr. 80 ; glycerini, ℥i. ; aquæ ad ℥viii.

Next morning her temperature was 99·8°, rising in the evening to 101°; pulse 80; respirations 20. She had slept towards morning, felt much easier, had hardly any pain except in the right shoulder. The swelling had almost left her joints. Ordered—salicylic acid to be continued.

On October 9th, her temperature was 100°, but there was no evening exacerbation; pulse 86; respirations 20. She had slept well all night, had sat up in bed to eat her breakfast, the swelling had entirely left her joints, and she was quite free from pain. Ordered—the salicylic acid to be given at intervals of three hours.

On October 10th, her morning temperature was 99°, evening, 98 8°; pulse 76 ; respirations 22. She was still perfectly free from pain and had a good appetite. The salicylic acid was now stopped; she had taken 210 grains. On October 12th, as there was a slight rise in temperature, though unaccompanied by pain or swelling of the joints, she was ordered 5-grain doses of the salicylic acid every three hours. The temperature having again fallen on the following morning, the salicylic acid was stopped when she had taken 40 grains. The temperature did not again rise, and she left hospital perfectly well on October 15th.

Record of Pulse, Respirations, and Temperature.

Day of Disease	Morning			Evening			Remarks
	Pulse	Resp.	Temp.	Pulse	Resp.	Temp.	
14	—	—	—°	108	24	102·6°	Salicylic acid 80 grains.
15	80	20	99·8	96	22	101·0	„ „ 60 „
16	86	20	100·0	80	22	100·1	„ „ 70 „
17	76	22	99·1	72	19	98·9	
18	74	21	99·0	70	22	99·2	Mist. sennæ co. ʒj.
19	76	19	99·4	74	25	99·8	
20	68	18	99·2	72	18	99·0	Salicylic acid 40 grains.
21	68	18	98·8	70	19	98·2	
22	68	20	98·8	—	—	—	Went home of her own accord.

CASE IV.—*Acute Rheumatism ; Second Attack ; Salicylic Acid from seventh day ; Apyrexial on ninth day ; total quantity used, 300 grains.*

Edward S., aged thirty, was admitted to the Meath Hospital on October 19th, 1876, complaining of swelling and great pain in his feet and knee-joints. A month ago he had got a severe wetting. A day or two afterwards he took a shivering fit and great pain came in his hands, feet, knees, and hips—his hands and knees swelled. He took to bed at once, and after lying for a fortnight, felt so much better that he got up, but, a week afterwards, had to go to bed again, as the pains in his joints returned, and his feet and knees swelled. He has now been lying for a week, and has been suffering intense pain, which is getting worse every day. Up to a month ago, he was very healthy, and had never suffered from illness, so far as he can remember. He had never previously suffered from rheumatic fever or from any sort of rheumatism.

On admission, on the evening of October 19th, his temperature was 103°; pulse 120; respirations 28. His left knee and ankle were very red, and much swollen; his right knee and ankle were swollen, but not red. None of the other joints were affected. On examining the heart, a systolic basic murmur was heard, very well marked over the pulmonary artery, and greatly intensified over a slight area of dulness which existed to the left of the manubrium, just below the sterno-clavicular articulation. He was at once put on 5-grain doses of salicylic acid every hour.

On October 20th, his morning temperature was 100°, evening 99·1°, pulse 116, respirations 30. He had slept but little. The pain in his joints was greatly relieved, and the swelling had somewhat subsided.

On October 21st, the morning temperature was 99·1°, evening 98·1°, pulse 84, respirations 27. He had slept almost all night, had no pain whatsoever, and the swelling had altogether left his joints. There was no change in the character of the cardiac murmur, but the area of dulness to the left of the sternum had almost disappeared.

On October 22nd, his temperature was only 98·2°, pulse 86, respirations 24. The swelling had completely left his joints, and he was free from pain. The salicylic acid was now stopped. He had taken 200 grains.

On October 25th, as there was a slight rise in both his temperature and pulse, he was ordered 5-grain doses of salicylic acid every three hours. It was stopped when he had taken 60 grains. The abnormal area of dulness had now entirely disappeared ; the systolic murmur had greatly diminished in intensity, and sometimes could scarcely be heard.

From this until October 28th, the temperature remained perfectly normal ; but on the day named, as the morning temperature was 99°, he was ordered 5-grain doses of salicylic acid every third hour. When he had taken 40 grains, the temperature was again normal, and the salicylic acid was stopped. From this he went on well, and left hospital quite convalescent on November 3rd.

Record of Pulse, Respirations, and Temperature.

Day of Disease	Morning			Evening			Remarks
	Pulse	Resp.	Temp.	Pulse	Resp.	Temp.	
7	—	—	°—	120	28	103·1	Salicylic acid 60 grains.
8	116	30	100·0	90	29	99·1	„ „ 60 „
9	84	27	99·1	84	24	98·8	„ „ 80 „
10	86	24	98·2	82	24	98·8	
11	92	24	99·0	84	24	98·8	
12	90	24	98·8	94	22	98·6	
13	96	24	99·0	90	22	98·6	„ „ 60 „
14	92	20	98·8	80	19	98·8	
15	80	18	98·4	78	20	98·4	
16	78	18	99·0	80	19	98·6	„ „ 40 „
17	74	20	98·4	78	20	98·6	
18	72	18	98·1	76	18	98·4	
19	80	18	98·8	—	—	—	
20	84	22	98·9	—	—	—	} Sitting up in evening.
21	86	20	98·3	—	—	—	
22	80	18	98·6	—	—	—	Left hospital, Nov. 3, 1876.

CASE V.—*Acute Rheumatism arising from Cold; Treatment by Salicylic Acid (5 grains every hour) from seventh day ; Apyrexial in 30 hours.*

Annie N., aged nineteen, a domestic servant, residing at Rathfarnham, County Dublin, was admitted to the Meath Hospital on Saturday evening, November 4, 1876. She was supposed to be getting inflammation of the lungs. Her history showed that she had not been strong for some months, an obstinate cough and loss of flesh being prominent symptoms. The catamenia, however, were regular, but she suffered from headaches. On Tuesday, October 31, a very frosty day, she caught cold. The cough became intensified, and pain set in along her back. On Wednesday, November 1, she noticed her feet swelling, and they

became painful. There was no headache. When admitted to the hospital on the evening of the fifth day of her illness, the pulse was 124 ;. respirations 24 ; and temperature 101·8°. In my absence she was given a diaphoretic mixture. Next day (Sunday) it was found that she had not slept for three nights, owing to " pains in her bones." A patch of herpes covered the central portion of the upper lip. Her tongue was slightly dry, but little furred. She stated that she never had suffered from rheumatism. As her pulse, temperature, and respiration were falling, no special treatment was ordered ; but towards evening, with great increase of pain, the temperature rose again 2° to 101°. Ten grains of chloral hydrate gave her five hours sleep ; but next morning she was no better, and in great pain, unable to move her right arm, which had become affected.

Monday, Nov. 6th (seventh day).—Pulse 112; respirations 28; temperature 100·0°. Her ankles were swollen and painful, both shoulder-joints were " sore," and the right elbow was swollen, tense, and exquisitely painful. The first sound of the heart was prolonged almost into a soft blowing murmur. The affected joints were wrapped in cotton wool, and 5-grain doses of salicylic acid were ordered to be taken every hour, The first dose was given at 11 30 am., and after three doses she got relief. At night the temperature was half a degree lower than in the morning. She slept well, and awoke so free from pain that Mr. Blacker Powell, my clinical clerk, on entering the ward, found her twirling the affected arm over her head. She had taken 80 grains of the acid in all. It was continued until November 8th, when the total quantity taken amounted to 150 grains. On November 14th, she sat up for the first time, and there was not another bad symptom.

Record of Pulse, Respirations, and Temperature.

Day of Disease	MORNING			EVENING			Remarks
	Pulse	Resp.	Temp.	Pulse	Resp.	Temp.	
5	—	—	°	124	24	101·8	
6	100	22	99·2	102	24	101·0	
7	112	28	100·0	104	32	99·5	Salicylic acid ⎫
8	80	27	98·8	96	32	98·2	„ „ ⎬ 150 grains.
9	86	32	98·6	88	28	98·4	„ „ ⎭
10	80	26	98·3	75	28	98·4	
11	78	28	98·3	74	30	98·3	
12	74	28	98·2	76	24	98·0	
13	68	26	98·1	64	24	98·6	Tooth-ache in afternoon.
14	66	24	98·1	66	28	98·4	Sat up to-day.
15	80	20	98·6	76	24	98·7	
16	76	28	98·3	66	26	98·3	Sat up from 11 a.m. to 3 p.m.
17	76	24	98·7	70	26	98·8	
18	—	—	—	76	28	98·7	Sitting up this morning.
19	80	26	98·3	—	—	—	Convalescent.
20	—	—	—	—	—	—	Left hospital.

CASE VI.—*Subacute Rheumatism; second attack; Salicylic Acid on fourth and fifth days; rapid convalescence;* 160 *grains in all of Salicylic Acid taken.*

John D., aged thirty-eight, a law-clerk, married and father of two children, was admitted to the Meath Hospital on November 10, 1876. He stated that twenty years ago he was exposed to a wetting, in consequence of which he had "muscular rheumatism" in his legs, but he added that he never had "rheumatic fever." On Wednesday, October 11—that is, four weeks before admission—he felt that he had caught cold while sitting in a fireless office on a very inclement wet day. A "stinging" in the left groin was the first symptom. There was no swelling in the groin, or redness, or tenderness on pressure. On October 13, he could not put his legs under him; there was a numbness in the groin and his knees swelled. The pain was two days in reaching its height in the knees, and from them it passed to the ankles and through the arms to the hands. For nine days he was in great agony. He was treated with "potash." On October 21 he was much better, so that next day he got up, and on the 23rd felt so well that he went to business. He remained pretty well until Monday, November 6, when a similar train of symptoms to those which ushered in the former attack recurred. The left groin, shoulder, and elbow got so bad that, on November 8, he could not go to business.

Friday, November 10 (fifth day of the second attack).—Pulse 76; respirations 20; temperature 98·8° just after getting into bed. Tongue rather dirty, but moist. No cardiac lesion. The affected joints were the left elbow, wrist, and fingers, all of which were considerably swollen and very painful; the left knee and ankle, which were swollen without pain. The left shoulder was nearly well. Salicylic acid was given in 5-grain doses every hour, commencing at noon. He fell asleep about 4 p.m., and afterwards slept almost through the night. By 9 a.m. next day he had taken 70 grains of the acid. The swelling about the affected joints (which had been encased in cotton-wool) was greatly diminished, and he could now freely move the left arm.

Record of Pulse, Respirations, and Temperature.

Day of Disease	MORNING			EVENING			Remarks
	Pulse	Resp.	Temp.	Pulse	Resp.	Temp.	
5	76	20	98·8 ?	68	23	98·8	Temp. taken just after he lay down in bed—salicylic acid.
6	72	18	98·0	72	20	98·9	
7	66	18	98·3	80	20	98·8	
8	64	20	98·2	82	20	100·0	Bowels confined—temp. 98·8° at 8 30 p.m.
9	68	18	98·2	72	24	98·2	
10	72	18	98·2	74	19	98·1	
11	68	20	98·2	—	—	—	
12	72	18	98·2	—	—	—	Left hospital.

There is nothing further to note in the case, except that a transitory feverish movement on the eighth day was apparently due to slight constipation. He left hospital on the twelfth day of his second illness—the eighth day of his sojourn in the wards. The quantity of acid used was 160 grains.

In attempting to estimate the value of salicylic acid in the foregoing cases, it will be necessary to contrast them with previous observations of acute rheumatism treated by other methods. From May, 1875, to April, 1876, ten patients suffering from this disease were under treatment in my wards. Case I. was a young woman, aged twenty-three, who spent twenty-two days in hospital under an attack of subacute rheumatism. She also had albuminuria and anasarca dependent on cardiac disease. Case II. was a young man, aged seventeen, who spent forty-one days in hospital, the temperature ranging about 100° almost throughout, except for two days, during which dangerous collapse from aortic valve disease placed him at death's door. He had been ill for three weeks before admission. He was treated by opium and Dover's powder with stimulants. Case III. was a mild attack in a girl, aged thirteen, admitted on the fifteenth day. Although the subject of mitral valve disease, she became speedily better, being only eight days in hospital. Case IV. was a young man, aged seventeen, who came in on the sixth day with temperatures of 101°. He spent forty-two days in hospital, dating from October 2, 1875; and was still feverish when he left. Dover's powder, iodide of potassium, acetate of potash, diaphoretics, and diuretics were all employed to combat the disease, and with but equivocal success. Case V. ran a course of twenty-six days in hospital. The patient, a girl, aged twenty-six, had endocarditis and pericarditis. Her illness began like Bernard C.'s, as given above. Battley's solution, morphia, calomel and opium, and tincture of aconite were the chief drugs used in her illness. Case VI. occurred in a man, aged forty-eight. He was thirty-four days in hospital from December 9, 1875. In his case we gave tincture of ferric chloride a full trial. Case VII. was admitted December 10, 1875, on the sixth day. The patient was a law-clerk, aged twenty-six. The temperature rose progressively for four days after his admission. He also got ferric chloride at first, and was afterwards treated by opium, &c. He spent forty-two days in hospital. Case VIII. was one of subacute rheumatism in a man, aged thirty. He stayed under observation for fifteen days only. Case IX., a girl, aged sixteen, who passed through a sharp attack, and was free from fever in a fortnight from the appearance of the symptoms. She was given ferric chloride and opiates. Case X. was one of very mild (subacute) rheumatic fever—yet the patient, a young man of seventeen, spent three weeks in hospital.

These ten patients spent an average period of 26·6 days under treatment

in hospital. The *seven* cases treated by salicylic acid remained in hospital for an average period of 12·4 days. If we regard Bernard C.'s two attacks of rheumatism as but one, and his sojourn in hospital as 31 days, the average duration of the stay in the wards of those who were given salicylic acid is 14·8 days. We thus see that, making every allowance in favour of the older methods of treatment and against salicylic acid—for we take no account of the fact that most of the patients treated by it were purposely kept under observation for many days after the symptoms had disappeared—the mean duration of the stay in hospital is now only about one-half what it used to be.

Again, an examination of the clinical charts in the cases shows that under the ordinary methods of treatment the average number of " days of pyrexia," or days on which the axillary temperature reached or exceeded 99° Fahr. was 19·3. Under salicylic acid, on the contrary, it was 4·6 days, or—counting Bernard C.'s illness as but one case—5·5 days. So that the symptom of feverishness was but *one-fourth* as persistent in the second series of cases as it had been in the first.

One striking effect of salicylic acid is the very speedy relief it affords to the patient's sufferings. Those who a short time previously were unable to move hand or foot, without excruciating agony, are shortly relieved of pain, and generally fall into a deep refreshing sleep. In this way the remedy acts as an anodyne and hypnotic, which is far more efficacious and satisfactory than opium.

And not only is pain relieved, but all redness and swelling quickly disappear from the affected joints. In one case measurements of a swelled elbow were accurately taken, and it was found that the normal circumference of the articulation was restored within 24 hours (Case V).

As to the method of administering the acid, a few words are required. First, with regard to the dose—in Case I. 10 grains were given every *third* hour, because the treatment was commenced in the evening, and I did not wish to imperil the patient's chance of a good night by wakening him every hour to give him medicine. Afterwards, the almost routine dose was 5 grains every hour by day, and at longer intervals by night. This comparatively small dose, frequently repeated, acted satisfactorily, never disagreed with the patients, and was not objected to by them. The total quantity taken in 24 hours seldom or never exceeded 80 grains. No toxic symptoms were noticed, except slight " singing" in the ears on one occasion. Generally, the patients took a mouthful of water or of cold tea after each dose. They did this to prevent a somewhat burning sensation in the throat, which occasionally followed the draught. In connexion with this I may mention that, in a case of scarlatina, salicylic acid acted very beneficially on the sore throat—the patient expressed himself better after each dose, and the throat became speedily well.

Secondly, as to the form in which the medicine was given. The

powder, which was originally employed in Germany, was deemed unsuit-
able, for obvious reasons. The well-known insolubility of the acid in
water did not, however, influence us much in prescribing, and the formula
I adopted, for the most part, was this :—

> " ℞.—Acidi Salicylici, gr. 80 ;
> Glycerini, ʒj. ;
> Aquæ Ferventis, ʒvij.   M.
> " Fiat mistura.   Signa: ' One tablespoonful every hour.'"

Latterly, the formula proposed by Dr. G. F. Duffey[a] has been
followed with satisfactory results.   In this case the salicylate of ammo-
nium was found to give the characteristic reaction in the urine with a
weak solution of ferric chloride.   A mixture of salicylic acid and milk
was tried, for the sake of experiment; but it did not succeed.   This
mixture has exactly the appearance of buttermilk and the taste of
sweetened buttermilk.   I have not used salicin in acute rheumatism ; but
as an antipyretic in enteric fever, &c., I have given it tolerably frequently,
and the results prove that it is decidedly inferior to quinine.

To enter here into the chemistry, the chemical relations and history
of salicylic acid, would be foreign to the purpose of this paper.   Nor is
any such reference required in the presence of such an admirable *résumé*
on the subject as my friend, Dr. George F. Duffey,[b] has compiled.   In
answer to a query put by me, Professor Emerson Reynolds wrote me
the following letter :—

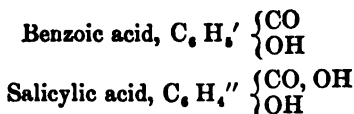> "52, LEESON-STREET, DUBLIN, *Nov. 25th*, 1876.

" DEAR MOORE,—I do not find that any relationship has been estab-
lished between salicylic and lactic acids.   Bertagnini has shown that
salicylic acid, when administered medicinally, appears in the urine partly
in the free state, but in chief proportion as salicyluric acid, a body which
resembles hippuric acid in constitution, and bears the same relation to
ordinary salicylic acid that hippuric acid bears to ordinary benzoic acid.
The following formula makes this plain :—

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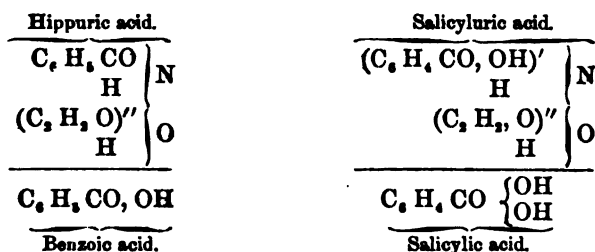
[a] *Vide* Brit. Med. Journal.   November 4th, 1876.   Page 587.
[b] See Irish Hospital Gazette.   May 1st, 1875.   Vol. III.   Page 138 *seq.*

Or, more fully:

$$\text{Benzoic acid, } C_6 H_5{}' \begin{cases} CO \\ OH \end{cases}$$

$$\text{Salicylic acid, } C_6 H_4{}'' \begin{cases} CO, OH \\ OH \end{cases}$$

" Salicylic acid, however, is very likely to form conjugate acids, and it may do so with lactic acid. It would be, at least, interesting to examine the urine of one of your patients after dosing with the salicylic acid.

<div align="right">" Yours sincerely,</div>

<div align="right">" J. E. REYNOLDS."</div>

At the first opportunity, I hope to act upon Professor Reynolds' kind suggestion.

Before concluding, it is right to state that since the foregoing cases were recorded, two patients in the hospital have been taking salicylic acid for rheumatism. One, a man, aged thirty-eight, came in on November 22, with acute rheumatism affecting the ankles, knees, and wrists. His temperature was $101 \cdot 2°$. He was also suffering from a severe attack of general bronchitis, supervening on a chronic bronchial catarrh. He took 85 grains of salicylic acid, when profuse diaphoresis and the urgency of the chest symptoms obliged me to withhold the acid and to treat him chiefly for acute bronchitis. However, the rheumatic swelling and pains subsided, and on the 1st of December he was progressing favourably. The second case was that of an elderly woman, the subject of chronic rheumatism. She was treated for some days with "Chelsea Pensioner," when suddenly an acute rheumatic attack in the joints of the left hand set in and yielded readily to salicylic acid.

If it is fair to base any definite opinion on so few observations, the conclusions to which my limited experience of salicylic acid have led me to are these :—

1. Salicylic acid appears to be a valuable and almost specific remedy in the treatment of acute rheumatism.

2. After the administration of a few moderate doses, of 5 grains each, given at hourly intervals, a marked amelioration of the symptoms usually occurs. Thus, the temperature and pulse begin to fall, the swelling and pain of the affected joints subside, and the patient sleeps.

3. The above doses—*i.e.*, of 5 grains each, are quite sufficient to produce an impression on the disease, while the patients make but little complaint either of the frequency of the dose or of the taste of the medicine.

4. When pushed far, it sometimes causes singing in the ears and diaphoresis. Under these circumstances its administration should be temporarily suspended.

5. To prevent relapse, it should be given for some days, but at gradually lengthening intervals.

6. Finally, as to its probable action as a preventive of the dangerous cardiac lesions of acute rheumatism, I can only endorse the words of Dr. Coates, of Belfast, in a recent paper:—" I think it can hardly be denied that medicines which cut short the disease, as I believe there can be no doubt it does, must render the liability to these complications less."

The CHAIRMAN said there was no disease in which a greater variety of treatment had been employed than acute rheumatism. In the year 1830, when he was a student in the Meath Hospital, it was customary to use tartar emetic followed by colchicum. After the epidemic of cholera in 1832, a new phase of treatment presented itself, which included calomel, opium, and blistering. After an interval that phase was superseded by the alkaline treatment. He had had no experience of the use of salicylic acid; but, as regarded the shortness of the period in which they proved efficacious, the other remedies bore no comparison with that brought forward by Dr. Moore. If Dr. Moore's experience should be borne out hereafter, the line of practice suggested would be most valuable.

DR. MCVEAGH said he had lately treated a case of acute rheumatism with salicin alone. The patient was a foreigner, of about forty years of age, and of good healthy constitution. This was his third attack, he having had two previous ones, each of about six weeks' duration. The attack was a severe one. He gave him half a drachm of salicin four times a day, and the effect was marvellous. In a fortnight he was perfectly well. He had no cardiac symptoms.. In his previous attacks he had been treated with calomel, opium, and blistering.

DR. GRIMSHAW stated that he had had considerable experience in the use of salicylic acid in the treatment of acute rheumatism, and a slighter experience of its effects in some other diseases. When first introduced into medicine, the acid had been used by him at Steevens' Hospital before it was obtainable in the Dublin drug market, some salicylic acid having been made for him by Dr. Chichester Bell, the lecturer on Chemistry to the Medical School of Steevens' Hospital. With regard to the question at present immediately under discussion—namely, the use of the drug in the treatment of acute rheumatism, Dr. Grimshaw had employed it in about twenty cases, and salicin in two cases; five cases were at present under his care, three under treatment with salicylic acid, and two under salicin. Dr. Grimshaw's experience quite agreed with that of Dr. Moore. Some of Dr. Grimshaw's cases were met with very early in the disease, and were at once relieved by the use of the

acid. The temperature greatly fell, and the pains vanished. The recovery of these cases early in the disease was so remarkable that Dr. Grimshaw was at first almost inclined to doubt whether, if they had been left to themselves, they would not have recovered equally well. This doubt, however, was removed by the effect of the acid on more severe cases. In several of these, where the disease had been established for ten or fourteen days before the patient came under treatment—in all of these cases, with one exception, the fever and pain subsided quickly under the use of the acid. The average period for the disappearance of the fever and tenderness of the joints was five to six days after the commencement of the treatment. In a few cases opium was given in moderate and single doses at night to produce sleep, but in no case were large and frequent doses of opium employed. The joints were usually packed in cotton wool. In cases (of which there were three) where pericardial inflammation had set in before the treatment commenced, the acid seemed to have little effect on the cardiac complication, although the febrile symptoms diminished, and the joints became less painful. In one case endocardial and pericardial inflammation arose while the patient was taking the acid, and this case ran the usual " six weeks' " course. The dose Dr. Grimshaw usually employed was 20 grs. every third hour. Dr. Grimshaw considered it was not advisable to repeat any disagreeable drug oftener than was absolutely necessary. Dr. Grimshaw contrasted the mild course run by the disease when treated with salicylic acid, to that it pursued when treated by alkalies and opium. A man had had four attacks of acute rheumatism ; the first two attacks were treated by alkalies and opium, and ran courses of five and eight weeks respectively ; the second two attacks were treated with salicylic acid, and each terminated in complete convalescence within a fortnight. The patient in whose case the salicylic acid had failed, had since been treated in a subsequent attack with salicin, with complete success, all symptoms having subsided within a week. Dr. Grimshaw believed that, up to the present, salicylic acid and salicin had proved to be the most useful agents in the treatment of acute rheumatism.

Dr. HENRY KENNEDY said that in no case should they be more slow to draw inferences as to the duration of a disease than in that of acute rheumatism. Sir William Gull and Dr. Sutton had lately published cases in which they had given nothing but mint water, and in which cure was effected in eleven days. Some years since Sir Dominic Corrigan published a paper advocating the use of opium, and mentioned cases which he had cured in that way just as quickly as the cases mentioned by Dr. Moore and Dr. Grimshaw. It had been lately noticed in *The Lancet* that the acid had produced untoward effects in two cases. In one case alarming symptoms occurred like those that sometimes resulted from the

excessive use of quinine—namely, delirium, noises in the head, and so forth; and on the acid being withheld those symptoms disappeared. When the heart was affected cardiac symptoms remained after the joints had got well, because the effusion in the heart was of a different quality from that in the joints. If a knee-joint was attacked the effusion might disappear in twenty-four hours, but they could not expect so sudden a disappearance of effusion in the pericardium.

DR. DUFFEY observed that the mischievous effects which apparently resulted from the use of salicylic acid in one of the cases mentioned, had been traced to the use of an impure acid. The acid used by Dr. Tuckwell was found, on analysis, to contain carbolic acid. It was to this impurity in the drug, as had been pointed out by previous observers, that the symptoms of poisoning occasionally observed after its administration were due.

DR. W. G. SMITH said a notion prevailed that there was a similar connexion between salicin and salicylic acid to that which existed between tannin and tannic acid; this was a mistake. There was no connexion between salicin and salicylic acid, save an extremely remote chemical one. The truth of the assertion made by some German writers that salicin was converted in the system into salicylic acid, was extremely doubtful.

DR. J. W. MOORE, in reply, disclaimed any originality as respected the mode of treatment he advocated. He had been only following an example set in other parts of Europe. In the cases he had mentioned no special examination of the urine was made. Perspiration disappeared rather speedily with the subsidence of the other symptoms of feverishness, pain, and swelling of the joints; and, coincidently, there was an increase of the urinary secretion. It would be seen that the periods of cure in his cases were shorter than those of the cases mentioned by Dr. Kennedy. They were shorter by one-third than those of the cases in which opium was used, and more than twice as short as those of the cases treated by mint water. He was not, however, a believer in the thoroughly specific action of any medicine.

DR. J. MAGEE FINNY read a paper on "Cases of Chorea." [It will be found at p. 30.]

DR. MACSWINEY said that, as Dr. Finny had confined the scope of his paper to the treatment of chorea, he had shut out a number of important points on which he (Dr. MacSwiney) would like to elicit information. One of these was the pathology of the disease, which was in an obscure

state. Dr. Dickinson, in a paper read before the Medico-Chirurgical Society of London, submitted 22 cases of fatal, and 70 of non-fatal, chorea. In the fatal cases there was universally present a dilated condition of the blood-vessels of the brain and spinal cord, and in many cases effusion of blood into the brain and spinal cord. The mortality also appeared to be greater in the English cases than in those that had occurred in this country. In the English cases rheumatism had been an extremely common antecedent and sequence of the chorea; and the relationship between them was one of the most mysterious peculiarities of the affection in question. If the dilatation theory were correct, the efficacy of the cold of ether-spray and the peculiar therapeutic effect of strychnine could be understood. It had been recently stated that there were grounds for believing that hypermetropia occurred in all cases of chorea, and that the cure of the chorea could be effected by using glasses to correct the hypermetropia.

DR. DUFFEY remarked that the theory Dr. Steevens had advanced in the New York Academy of Medicine, as to the causation of chorea—viz., that it arose from irritation dependent upon anomalous reflection of the eye, and in a very large proportion of cases to hypermetropia, had not, apparently, received much confirmation. Cases of chorea have been reported since the publication of Dr. Steevens' paper, in which the eyes were examined ophthalmoscopically, and no error of refraction was detected in them.[a]

DR. FINNY, in reply, said he had expressly excluded from his paper the consideration of the causes and the pathology of the disease.

The Society then adjourned.

USE OF THE FORCEPS IN DELIVERY.

DR. JURNEY, Professor of Obstetrics in Ohio, believes the ordinary method of using the obstetrical forceps a very frequent cause of laceration of the perineum. He makes little or no traction during the pain; but, in the interval of the pains, he uses traction enough to prevent recession of the head of the child, holding it firmly against the perineum. This prevents a re-contraction of the perineum, and converts a violent intermittent distensive force into one which is slowly acting and persistent. Under this moderate but constant pressure, the perineum surely and safely dilates, and laceration is avoided. The reversed rule, therefore, in the use of the forceps will stand thus—make no traction during the pains; let the traction be made in the interval of the pains.—*Ohio Med. and Surg. Journal.*   S. W.

[a] *Vide* New York Med. Record, Aug. 12 and Oct. 14, 1876.

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

President's Inaugural Address.—The PRESIDENT delivered the following Address:—

GENTLEMEN,—My first duty, in taking this chair as President of the Ulster Medical Society, is to return you my cordial thanks for the honour you have conferred upon me in electing me to the office. I can un-affectedly say that I would have preferred that your choice had fallen upon some one else who had higher claims than I have. Indeed, I at first hesitated to accept the position, but on hearing that my appoint-ment was desired by a number of my professional brethren, I felt that I would be wanting in a due appreciation of their kindness if I refused so flattering a compliment. I will take this opportunity of making a few observations upon the value of such an institution as the Medical Society, if worked properly. Our worthy ex-President, Dr. Wheeler, notwith-standing the demands upon his time, has shown us a good example in his regular attendance at, and in the interest he has taken in, the meet-ings during the past Session.

The friction of mind with mind, as iron upon iron, sharpens our faculties and promotes our professional improvement. No one can attend our meetings—listen to clinical facts accurately recorded—see disease or the effects of medical or surgical treatment in the living man—examine morbid specimens in which are exhibited the destructive effects of injury or pathological processes—keep himself informed in medical literature by our circulating library—and fail to find his mind more enlightened and his feeling of self-confidence increased when he holds the scales of life and death in his hands. Knowledge in our profession, as in every other department of human activity, is endless, and, no matter how great our information, we shall always have much to learn. Some one has written, " In earth there is nothing great but man; in man there is nothing great but mind;" and we, to whose care is committed this wonderful microcosm, may well magnify our office, but it all the more becomes us to develop, by every means within our reach, our

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intelligence, skill, and proficiency. The course of nature is doubtless unalterable; but the Almighty has conferred mind on man enabling him to be an interpreter of nature, and by his knowledge of natural laws capable of modifying one law by another. Bacon said that " man com- mands nature by obeying nature." Nature makes water flow down, but man makes it flow up a hill to fill a reservoir, by modifying, not inter- rupting, the laws of nature. Likewise in scientific medicine, if we get clear ideas of nature's laws in man and in his environment—or, to go more into detail, if we study man mentally and physically, in health and in disease, acquaint ourselves with his surroundings, the air he breathes, his food, his drink, the accidents to which he is exposed, what is favour- able and what is unfavourable to his health—abundant opportunities will be afforded us of bringing to pass results which, but for man's interven- tion, unaided nature would not bring to pass.

One word upon another advantage of our Association, and I have done. It is as true of individuals as it is of nations, that with the increase of relations and mutual knowledge suspicions and enmities decrease. Our meetings here for the advancement of medicine and surgery, our discussions, and each of us making it our motto, " Gladly would he learn and gladly teach," must tend to the promotion of kindly feelings and good-will in the members of the Society :—

> " Friendship is no plant of hasty growth,
> Though planted in esteem's deep-fix'd soil :
> The gradual culture of kind intercourse
> Must bring it to perfection."

Notes of Cases of Acute Rheumatism, treated by Salicylic Acid ; with a few Remarks on the Acid, its Administration, &c. By STANLEY BERES- FORD COATES, L.R.C.P. & S. Ed. ; Senior Resident Surgeon, Belfast Royal Hospital.

CASE I.—James Drain, a healthy man, aged about eighteen years, a ticket-collector on a railway, admitted Thursday, 21st September, 1876 ; had been ill and totally unable to work for six days previous to his admission.

On admission, his hip, knee, ankle, elbow, and right wrist-joints were affected, tender and swollen, the slightest motion causing extreme pain ; tongue furred ; copious acid perspirations ; had a mitral murmur, which, however, we have reason to believe, existed previously.

At 12 noon, on 21st September (day of admission), his temperature was 102·9° (almost 103°) ; pulse 83 (normal pulse 55). He was ordered salicylic acid, grs. xv. every hour ; this was continued, with but one intermission of an hour, for 14 hours, when the temperature had fallen to 99° ; pulse 75. When he had taken the tenth dose the pains had almost disappeared, and at the twelfth dose he could move the limbs

freely. On the evening of the 22nd September (day after admission) he had a slight return of the pains, which, however, disappeared at once on the administration of a few doses of the acid. On September 26th he was ordered quiniæ sulph. grs. ½ three times a day, and he left hospital on 3rd October quite well.

CASE II.—Lizzie Gamble, a healthy woman, aged thirty, a domestic servant, admitted on 12th August, 1876. She was able to trace her present illness to a severe wetting received about two weeks previous to her admittance.

On admission she was suffering intense pain—knee, ankle, shoulder, and elbow-joints were affected; perspiring freely; tongue furred; bowels constipated; no heart affection. She was put under treatment at 10 30 p.m. on 12th August, and the joints having been bound up with cotton wadding, she was ordered grs. xv. salicylic acid, to be repeated each hour for six hours. At 10 30 p.m. the temperature was 100·8°; pulse 98; and at 3 30 a.m. on the next day, at which time the sixth dose of the acid was given (five hours from its commencement), the temperature was 98·4°; pulse 92. After she had taken two or three doses of the medicine the pains began to disappear, and at the sixth dose she was able to move all the joints freely. She convalesced satisfactorily, getting a few doses of the acid at more remote intervals, then sulphate of quinine for a few days, and left hospital well on 2nd September.

CASE III.—Sarah J. Thompson, a healthy woman, domestic servant, aged twenty-three, admitted 15th September, 1876; had been five days ill and confined to bed previous to her admission.

On admission, knees, ankle, shoulder, elbow, and wrist-joints were affected, the pain being of so excruciating a character that she was incapable of the least motion—all the symptoms of acute rheumatism being exceedingly well marked in her case. The joints were enveloped in cotton wool, and at 9 p.m. she was put under treatment, being ordered grs. xv. salicylic acid every hour till eight doses had been given. When the treatment was commenced, Sept. 15, 9 p.m., temperature 103°; pulse 110. Sept. 16, 9 a.m., temperature 100°; pulse 105. Sept. 16, 9 p.m., temperature 99°; pulse 96. When she had taken three doses the pains began to disappear, and when she had taken eight doses were almost gone.

An incident in this patient's case illustrates very well "the rapidity with which the salicylic acid acts"—viz., at 9 p.m. on the 15th Sept. she was unable to make the slightest movement, and at 6 a.m. on the 16th, nine hours after, she, in the temporary absence of the nurse from the ward, got out of bed and walked across the floor—just as she expressed it—"to see what she could do."

She had a slight relapse on the 18th, which was easily cured by a few 10-grain doses of the acid. She convalesced satisfactorily, and left hospital on 7th October.

I will now take the liberty of reading a few general remarks on salicylic acid, its doses, mode of administration, &c.

It is obtained from willow bark, and is only slightly soluble in water, but is soluble in rectified spirit, and in solutions of alkaline carbonates, forming in the latter case salicylicates. Indeed, it is used most extensively in the form of the salicylicate of soda by Dr. Frerichs, of Berlin, Professor Gairdner, of Glasgow, &c. When pure, salicylic acid consists of shining white needle-shaped crystals, which have a very slight smell— indeed, when perfectly pure, no smell. The advantage of the pure acid is that it can be given internally in considerable doses without any of those unpleasant results which have followed the use of the ordinary commercial acid.

As to the dose in the above cases, the quantity usually given was 15 grains every hour for 8 and in one case for 14 hours. In cases of typhoid fever, Professor Frerichs gives 5·0 grammes (77 grains) in a single dose, each day at 12 noon, with a view to counteract the usual evening rise, with the result of reducing it in 80 per cent. of the cases. Very large quantities have been taken without any deleterious effect. In one case, that of a patient of Dr. Stricker, of Berlin, 22 grammes (340 grains) were taken in 12 hours.

Large doses in some patients produce symptoms allied to cinchonism— viz., buzzing in the ears, and even temporary deafness. The latter occurred to a slight extent in two of the cases treated here. However, the symptoms disappeared very soon after the discontinuance of the medicine.

It may be ordered as a powder; here it has almost exclusively been used, prescribed with a little tinct. of orange and water, giving particular directions as to the bottle being carefully shaken before each dose is measured out. I think it is a matter of importance that it should be given in a proper state of dilution, and have found that when three or four ounces of water were given with each dose the danger of nausea was reduced to a minimum—indeed, in none of the above cases did it give any trouble. Milk should not be given, either immediately before or immediately after any of the doses of the acid, as it seems particularly to disagree.

It seems to make little difference whether the pure acid be given or one of its alkaline combinations, as the salicylicate of soda; the latter has not, however, been tried here.

Salicin has been used very extensively by Dr. T. Maclagan, of Dundee, who speaks of it in the highest possible terms. He says of it that "the

relief of pain is always one of the earliest effects produced;" that "in acute cases relief of pain and fall of temperature generally occur simultaneously;" that "in subacute cases the pain is sometimes decidedly relieved before the temperature begins to fall;" that "the more acute the case the more marked the benefit produced," &c. I believe these observations are equally true of salicylic acid. Dr. Maclagan gives in an average case grs. xv. to grs. xx. of salicin every hour.

As to the effect of either of these drugs on cardiac complication, so frequent in acute rheumatism, I think it can hardly be denied that medicines which cut short the disease, as I think there can be no doubt they do, must render the liability to these complications much less.

In conclusion, I may state that in a very few cases the administration of these medicines has been alleged to have produced rather alarming symptoms. In all probability, in these cases the symptoms were due, not to the acid, but to its being impure. It is a well-known fact that the impure acid contains carbolic acid; also that in poisoning by carbolic acid the urine has a characteristic olive-green colour; that there is stertorous breathing, a burning sensation in the mouth, fauces, and stomach, followed by vertigo, and a feeling of intoxication, great lowering of temperature, and feeble pulse. Now, in the cases which have been reported "of symptoms of poisoning produced by salicylic acid," and markedly so in the cases recorded in this week's *Lancet* by Dr. Tuckwell, of Radcliffe Infirmary, Oxford, the above are almost the exact symptoms which he ascribes to the use of the salicylic acid, but which, I think, we have ample proof to show are due to its impurities, particularly the carbolic acid.

Dr. Core had tried the acid in two cases, both of which were very satisfactory. He gave in six doses in one case 120 grains, and from what he saw and read he believed it was the best remedy for the disease.

Dr. Esler tried the acid in one case, and was satisfied with its result, which was most marked.

Dr. Spedding used the acid in one case only, where he could narrowly watch the result, though he had given it in several dispensary cases, where he could not answer for its action. He found in the case referred to that the acid produced, in 10-grain doses every hour, very alarming *apoplectic* symptoms, and he was satisfied thoroughly as to the purity of the article; and he attributed the symptoms to the toxological effects of the acid.

Dr. Beck, senr., had very large experience in the treatment of acute rheumatism, and he, at various periods, believed he had found the "true

remedy," but after a time he became convinced he must resign each for
another, until now he could hardly say which was the most favourable;
on the whole, he leaned to the alkaline treatment.

Dr. MERRICK thought, from the pathology of the disease, the alkaline
treatment was the safest.

Dr. J. MOORE thought the treatment of the disease was anything but
satisfactory. He contrasted the vagueness of medical theories with the
definite results of surgery.

Dr. J. W. BROWNE was not satisfied with the efficacy of the acid in
acute rheumatism. He referred to the cases treated in Charing Cross
Hospital, and did not believe it had any effect in reducing the tempera-
ture.

Dr. WHEELER, who highly approved of the alkaline treatment, did
not agree with Dr. Moore and others, who advocated the expectant
treatment, and he believed much could be done in acute rheumatism.
The relief of pain by opium in his own case was very satisfactory.

Dr. WHITLA (Honorary Secretary) thought that before the efficacy of
a remedy could be thoroughly established the nature of the disease must
be carefully studied. Acute rheumatism was a disease which, in many
instances, was observed to abort or " cut itself short." This explained
the history of the treatment. Hosts of remedies have been hotly advo-
cated from time to time as specifics, which was owing to this peculiarity.
He believed no conclusion could be safely arrived at till a large mass of
evidence was collected.

Dr. GRIBBEN had used various remedies with varying success. The
alkalies were, in his hands, very successful. He agreed that many cases
of acute rheumatism did abort without any remedy, and if any happened
to be used it got the credit of having cut short the disease.

Dr. ROSS (President), in thanking Dr. Coates for his paper, said he
had appealed to the thermometer and pulse—undeniable indications in
the treatment of the cases. He had himself introduced the acid into the
hospital, and was satisfied with its results; but he did not believe the
best effects followed large doses frequently administered. He found
most benefit from 10-grain doses given every three or four hours.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

President—Thomas Hayden, F.K.Q.C.P.

Secretary—E. H. Bennett, M.D.

Myxoma.—Dr. Lombe Atthill said: The specimen I have to exhibit is an example of a myxomatous tumour occurring in the labium of a patient at present under my care in the Rotunda Hospital. She is a young healthy married woman, the mother of three children. She states that about a year ago she perceived that the left labium had become enlarged. At that time she was pregnant, and the tumour, according to her description, was not much larger than a walnut. After the birth of her child the swelling of the labium disappeared. This was, however, but for a time, for the labium soon began to increase in size, till at the end of nine months it had become so large as to cause her great inconvenience, and she came to Dublin to have it removed. On exposing the parts, the left labium was seen to overlap and entirely conceal the orifice of the vagina. It formed an egg-shaped tumour, which was very elastic to the touch, and conveyed the impression in the first instance of being an hernia. Some indistinct fluctuation was also perceptible, the presence of which decided me to introduce a small trocar by way of exploration. No fluid, however, escaped. I therefore proceeded to remove the growth, which I presumed to be an encysted tumour of moderate size, but on dividing the integuments I soon became aware that it was of very large size, and deeply seated. The removal of this large tumour was a matter of very great difficulty. On account of the depth to which it penetrated, I was afraid to use the blade of the knife, and had to break down its attachments with my finger and the handle of the knife, till at last I perceived that my finger was in close proximity to the tuber ischii. After a tedious operation, occupying more than half an hour, I succeeded in separating from its attachments, and virtually enucleating, this large tumour, which I now show. You see it is quite soft, and fills both my hands. On examination by the microscope, it proved to belong to the class known as myxomatous tumours growing from the mucous tissues, and, as is usually the case, of a benign character. At the time of removal it was somewhat firmer than it is at present, but not much so. It was contained in a not easily demonstrable capsule. There were evidently no large blood-vessels running into it, for the amount of hæmorrhage was very small indeed. The cavity which remained after its removal was so large that it took an enormous quantity of cotton to

fill it up. On the third day there was a considerable amount of secondary hæmorrhage, which was restrained by the introduction into the cavity from which the tumour had been removed of cotton saturated with the perchloride of iron. This has been the sole untoward symptom. The patient is now nearly well.—*November* 25, 1876.

Laceration of the Brain with Compression.—DR. BOYD said: This is the brain of a man who was admitted into St. Michael's Hospital, Kingstown, on the 31st of last month. He had fallen a distance of nine or ten feet from a ladder in Lower George's street, and his head had come into contact with the flagged footway below. He was brought to the hospital insensible. When I saw him half an hour afterwards the insensibility was passing away, and he was able to complain that he had received bruises of his shoulder and wrist. I found him suffering from a lacerated wound of the forehead, just above the right eye, and corresponding to the frontal eminence. There was some bleeding from it, but not to any great extent, and no fracture could be discovered. Water-dressings were applied, and a bandage to his head. He slept very well that night, and on the following day had somewhat recovered from the shock he had received. He made no complaint of headache. The water-dressing was left on the wound, and he continued to progress favourably. On the third day, when the dressing was removed from the wound, suppuration seemed to be going on, and a preparation of terebine and oil was applied. He continued to progress favourably, took nourishment every day, sat up in bed, and was able to answer questions without any particular complaint of pain in the head. On the ninth day the wound was granulating and closing up, and I considered him so well that I allowed him out of bed. He walked about the ward the next day, and then went down into the grounds of the hospital along with the other convalescent patients. He also had one or two pipes that day; he had been in the habit of smoking, and I did not see anything to prevent his doing so. On the evening of that day he complained of some pain in his head when going to bed. During the night it was noticed by the other patients in the ward that he slept rather heavily, and when the resident saw him in the early part of the following day, the eleventh day after the date of the injury, he found him showing symptoms of compression; and, when I saw him, these symptoms had increased. He was unconscious, his pupils were contracted, his pulse full and soft, and perspiration breaking out about his head and body—the symptoms of compression becoming more and more profound each hour. During the early part of the day he had retention of urine, but later in the day the sphincters relaxed. On the evening of the following day he died. On making a *post mortem* examination, I found the wound in his forehead almost closed; but on removing the calvarium the following symptoms

presented themselves:—Corresponding to the wound externally, the internal table of the frontal bone was driven in a quarter of an inch. This was pressing against the dura mater, but did not lacerate it. On the opposite side of the frontal bone, at the orbital plate, there was a fracture extending from before backwards to the body of the sphenoid. On opening the dura mater—through which I could see a good deal of effused blood—I found a large clot resting on the orbital plate of the frontal bone, and filling up a large lacerated cavity in the under portion of the right frontal lobe, which can be seen. The dura mater was adhering all round the laceration, which was about an inch in depth and two inches in length. The case is of interest, as showing the length of time that may elapse after injury to the brain without secondary hæmorrhage, and also the enormous amount of laceration that the brain may suffer without any suspicious symptoms, for he had no head symptoms, except those of concussion, which appeared after the injury, until the compression developed itself on the evening of the tenth day, or the beginning of the eleventh.—*November* 25, 1876.

Bursal Tumour.—DR. THOMSON said: This specimen was taken from a pensioner, who was admitted into the Richmond Hospital some weeks ago, complaining of a tumour in his right buttock. On examination I found a very much enlarged bursa, and a hardened mass, about the size of a large walnut, underneath the skin, but having an attachment deep down to the tuberosity of the ischium. Near the anus was a cicatrix, which resulted from the removal of a bullet. The man was in the Crimean war, and while taking part in the storming of the Redan was shot at the crest of the left ilium; the bullet traversed over the sacrum to his anus at the right side. He said he was quite sure it had been removed. I thought that this hard mass might contain the fragment of a bullet which might have been broken off. An exploring needle could not be forced into the small hard mass, and this seemed to strengthen the opinion that some foreign body was encysted there. The man being most anxious to have something done for the removal of the tumour, I operated on last Wednesday. I cut down first upon the small tumour, and exposed what was apparently a mass of fibrous tissue. It has not yet been examined with the microscope, but there does not seem to be any foreign body contained in it. In the course of dissection I opened the enlarged bursa, and from it issued a large quantity of fluid and some hundreds of the usual melon-seed bodies that we find in chronic bursæ. After I had enlarged the opening a little, there bulged out of it a number of white cord-like processes. On catching hold of them, they came out in loops about the size of the small intestine of a rabbit. I found these cords adherent to the cyst wall, a portion of which I dissected out with these attached to it. The walls are about an eighth of an inch thick.

Growing from the inner surface are four cords from two to three inches long, attached by their extremities to the opposite surfaces of the cyst. They are grouped in two pairs. My colleague, Dr. Harvey, is at present engaged in making a microscopical examination of a section of this specimen. At first sight these cords would seem to have been produced probably by the effused lymph produced by some limited inflammation that occurred in the cyst. This becoming distended with fluid, these bands of lymph were extended and became organised, and may have ultimately attained their present perfectly smooth and round structure— for they are round—by the constant action of these large masses of seed-like bodies. These bands in bursæ are rarely seen, but they have been noticed by Virchow and others.—*November* 25, 1876.

Urethral Stricture, with Enlarged Prostate and Cystitis.—MR. CROLY said : The specimen which I present to the notice of the Society is of very considerable interest, both surgically and in a pathological point of view. It is the bladder and urethra of a man who died a few days since in the City of Dublin Hospital. He was seventy-nine years of age, and had been admitted with all the symptoms of retention of urine. The bladder was enormously distended, and on examining him I found that he had what we suspected from his age—namely, a very considerable enlargement of the prostate gland. The relief of the bladder was the first consideration ; but on attempting to pass a " prostatic catheter," when the instrument reached the termination of the spongy portion of the urethra it became stopped. It was evident that the man had a very serious complication for the surgeon to deal with—namely, a tight stricture of the urethra complicated by enlargement of the prostate gland. Every surgeon is aware of the length and size of the instrument that has to be used in such a case, and also of the peculiar curve required in order to get over the prostate gland. It was, therefore, not easy to pass an instrument. However, I succeeded with a very small silver catheter, having a point slightly bent ; and when I got through a very tight stricture (the instrument having been " tilted " over the prostate gland), the bladder was unloaded, and relief was thus effected twice a day for some time. The urine drawn off presented the usual appearances so familar to the surgeon in cases of what is known as vesical catarrh, and the patient was accordingly treated with the object of relieving that peculiar condition of the urine, and at the same time of supporting his strength. A low form of fever, however, set in, of which the man soon died. I am indebted to Mr. Crozier, one of the residents of the hospital, for a careful dissection, which shows the condition of the stricture and also the interior of the bladder. It is a very remarkable example of the result of vesical catarrh. We find the bladder diminished in size, and its coats much thickened. We have that peculiar appearance

of its lining resembling the lining of the heart. There is also at the back of the prostate gland, in the region known as the trigone, a copious deposit of phosphates, and lymphy exudation adherent to the coats of the bladder. We further found an abscess between the bladder and the rectum, and the prostate gland very much enlarged. On examination of the kidneys, the ureters were found very considerably dilated, and a section of the substance of the right kidney disclosed a very large abscess. Thus we have a case which is of interest, both surgically and pathologically—surgically on account of the difficulty of introducing an instrument into the bladder, and pathologically from the extent of the disease. The situation of the stricture is another point of some interest. We know that in the time of Brodie, and other surgical authorities of his time, stricture was understood to be most frequently seated about the membranous portion of the urethra. But from the researches of Sir Henry Thompson, who examined nearly 300 specimens of the disease, it appears that organic stricture of the urethra is most frequently situated in front of the membranous portion of the organ, where it joins the spongy part, and even a little in its front.—*November* 25, 1876.

DYSPEPTIC ASTHMA.

AT a late meeting of the Berlin Medical Society, Prof. Hennoch detailed the histories of several cases of this affection occurring among children which had come under his observation. The symptoms were alarming dyspnœa, with pallor of the face and lividity of the lips, coldness of the extremities, small and extremely frequent pulse, superficial and very frequent respiration, and great mental apathy. The affection apparently depended upon disturbance of the digestive functions. There were in all of the cases some tumidity and tenderness in the epigastrium ; but in spite of the threatening symptoms, not the least indication of cardiac or pulmonary disease could be found on repeated and careful examinations. In one case, that of a child of nine months old, in whom there had been constipation and vomiting, great relief was afforded by the application of numerous dry cups to the chest, and recovery from the attack coincided with the eruption of an incisor tooth. The other patients were children of nine years, three months, and two years respectively, and all were relieved by the action of an emetic or cathartic. Prof. Hennoch, although sceptical at first, ultimately came to agree with the opinion expressed by Traube, who saw the first case in consultation—viz., that the disturbance in the stomach excited a reflex vaso-motor spasm in the small arteries, whence followed the coldness of the extremities, imperceptible pulse, stasis in the venous system and right heart, cyanosis, accumulation of carbonic acid in the blood, and dyspnœa. He therefore assigns the name *Asthma dyspepticum* to the affection.—*Berl. Klin. Woch.*, May 1, and *N. Y. Med. Rec.*

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, December 2, 1876.

Towns	Population in 1871	Births Registered	Deaths Registered	Deaths from Zymotic Diseases							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhœa	
Dublin, -	314,666	620	625	4	12	20	2	11	27	16	25·8
Belfast, -	182,082	503	363	—	—	14	1	21	12	14	26·0
Cork, -	91,965	195	162	—	—	—	1	2	1	3	22·8
Limerick, -	44,209	81	76	—	—	7	—	1	2	1	22·5
Derry, -	30,884	79	56	—	1	—	—	2	2	—	23·8
Waterford, -	30,626	61	47	—	—	—	—	—	3	—	20·0
Galway, -	19,692	37	29	—	—	—	—	—	—	1	19·0
Sligo, -	17,285	32	27	—	—	—	—	—	2	—	20·3

Remarks.

The death-rate was high in Belfast and Dublin, rather high in Derry, moderate in Cork and Limerick, and rather low in Sligo, Waterford, and Galway. The corrected rate in Dublin was 24·9. The death-rate of the four weeks was 24·7 per 1,000 of the population annually in London, 25·4 in Glasgow, and only 18·7 in Edinburgh. Fever, scarlatina, diarrhœa, and measles were fatal in Dublin; whooping-cough, scarlatina, and diarrhœa in Belfast; and scarlatina in Limerick. The epidemic of small-pox is unhappily making considerable progress in London, where 210 deaths were attributed to this disease against 74 in the previous four-week period. In Dublin also a local outbreak has occurred in the north and north-east districts of the city. The four deaths registered all took place in the Mater Misericordiæ Hospital.

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20′ N., Long. 6° 15′ W., for the Month of November, 1876.

Mean Height of Barometer, - - -	29·818 inches.
Maximal Height of Barometer (on 1st at 9 a.m.), -	30·453 ,,
Minimal Height of Barometer (on 14th at 9 p.m.),	29·173 ,,
Mean Dry-bulb Temperature, - - -	45·2°
Mean Wet-bulb Temperature, - - -	43·6°
Mean Dew-point Temperature, - - -	41·8°
Mean Elastic Force (Tension) of Aqueous Vapour,	·264 inch.
Mean Humidity, - - - -	88·5 per cent.
Highest Temperature in Shade (on 15th), -	57·3°
Lowest Temperature in Shade (on 9th), -	31·0°
Lowest Temperature on Grass (Radiation) (on 9th),	27·1°
Mean Amount of Cloud, - - -	73·0 per cent.
Rainfall (on 20 days), - - - -	3·614 inches.
General Direction of Wind, - - -	S.E. and W.

Remarks.

At 9 a.m. of the 1st the barometer read 30·453 ins., and the air was still and cold. A sharp frost occurred at night, but next day milder, cloudy weather set in. On the 8th a rapid fall of temperature and an E. wind, with frequent showers of cold rain, hail, sleet, and snow, supervened on a brisk rise of the barometer in Northern Europe. At Haparanda the barometer read 30·83 ins. at 8 a.m. This cold period did not last long, and the weather soon became mild and rainy, with areas of barometrical depression travelling from S. to N. and from S.W. to N.E. across the British Islands. On the 13th and 14th temperature rose to 77° at Biarritz, in France, where the thermometer had fallen to 32° on the 10th. In contrast to this, it fell to 13° at Haparanda, at 8 a.m. of the 18th. In Ireland a third cold period, also of short duration, commenced on the 26th; but no severe frost was experienced. The month was on the whole cloudy, damp, and mild, with frequent S.E. winds, and but little frost.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

THE INTESTINAL LESIONS OF TYPHOID FEVER.

DR. FREDERICK P. HENRY has contributed a paper to the Pathological Society of Philadelphia, reported in *The Medical Times* of that city (Aug. 19, 1876), in which he attempts to explain why it is that the intestinal lesions of typhoid fever gradually increase in severity as we approach the ileo-cæcal valve. The theory he advances is of a twofold nature. From the fact that the superior mesenteric vein has its origin in the lower portion of the ileum, he assumes—first, that there is in that portion of the tract a tendency, gradually increasing from above downwards, towards a mechanical hyperæmia. In addition to the peculiarity of vascular distribution, he adds, in the second place, an abnormal amount of peristalsis in this portion of the intestinal canal in typhoid fever. As to the effect of peristaltic contraction upon these lesions, he says:—Suppose a follicle two feet above the ileo-cæcal valve to have arrived at the height of the stage of medullary infiltration; at this moment a peristaltic wave traverses the canal; it may not be sufficiently powerful or long-continued to cause sloughing of the mass, but transfer it to the neighbourhood of the valve and the case will be different. The effect of a peristaltic wave gradually increases in severity as we approach the valve, owing to the gradually increased tendency to mechanical hyperæmia, and sloughing gradually becomes more extensive in the same direction. The second factor in Dr. Henry's theory, viz., increased peristalsis in a diseased condition of the ileum, has a bearing upon treatment. Small and frequently-repeated doses of opium, or any other drug that will allay spasm of unstriped muscle, will serve to restore the peristaltic function to a healthy condition, or reduce it to a degree somewhat below the normal. When laxatives are needed in typhoid fever, he would recommend those that excite the least amount of peristalsis. Castor oil, which is a powerful exciter of peristalsis, evidenced even in health by the griping which it causes, should, he thinks, be condemned as a purgative in typhoid fever. This purgative is the one almost invariably used in that disease, and largely from an odd association of ideas. Its physical properties as a lubricator have caused it to be regarded as soothing to the inflamed mucous membrane. Some drugs act as purgatives, chiefly by increasing peristalsis, whilst others seem to act principally by causing a watery secretion from the mucous membrane. Of the latter class are salines, and especially the sulphate of magnesium. The fact that sulphate of

magnesium does not increase peristalsis is vouched for by such experimenters as Vulpian and Legros, while the latter declares this to be true of the whole class of saline purgatives. When, therefore, it is necessary to employ purgatives in typhoid fever, he would recommend salines in decided preference to castor oil while enemata, as mere exciters of the function of defecation, are, he thinks, unobjectionable.

BROMIDE OF ARSENIC IN THE TREATMENT OF EPILEPSY.

Dr. Th. Clemens, of Frankfort-on-the-Main, has employed bromide of arsenic for twenty years in the treatment of diseases of the nervous system, and especially of epilepsy, and claims that he has obtained astonishing results with it. He uses the liquor arsenic. bromat., and gives one or two drops in a glass of water once, or, if necessary, twice daily. These minute doses may be given for months and even years, without producing the usual unpleasant effects of a long-continued arsenical course. All his cases of epilepsy have been markedly relieved and improved by this remedy, but in only two cases has it produced a complete cure. In many cases of incurable epilepsy, complicated with idiocy and deformities of the skull, the fits were reduced in number from twenty, in the twenty-four hours, to four or even two—a result that has been obtained by no other treatment. In connexion with the bromide of arsenic, an almost exclusively meat diet is advised. The patients should be as much as possible in the open air in the daytime, and their windows should be kept open at night. Unlike bromide of potassium, this remedy does not require to be given in increasing doses, and instead of interfering with digestion improves the nutrition and strength. Dr. Clemens has employed the following formula since 1859, and thinks that it ought to replace Fowler's solution, which is irrational in its composition and uncertain in its action. This solution becomes stronger with time; the chemical union of the bromide with the arseniate of potash becoming more and more perfect.—℞. Pulv. arsenic. alb., potass. carb. e. tartar., āā ℥ i; coque cum aquâ destil. ℔. ss. ad solut. perfect.; adde, aq. evaporat. restitutâ, aquæ destil. ℥ xij., dein. adde brom. pur. ℥ ij., refrigerat. stet per sufficient. temp. ad decol., S. liq. arsenic. bromat.—*Allg. Med. Central-Zeitung. and N. Y. Med. Record.* Sep. 29.

NERVOUS PHENOMENA IN CASES OF INTESTINAL STRANGULATION.

M. Berger has collected sixteen cases, which he divides into three groups:—Cramps, contractions, and convulsions. A fourth species may also be admitted—viz., syncope, of which one instance has been noted. Cramps are most frequently observed. They are, especially, common in the calf, sometimes in the arms, and all down the leg; in very severe cases they engage the wrists and fingers. The pains are so violent that they may mask those due to the strangulation. The variety of the

strangulation, crural or inguinal hernia, internal strangulation, &c., has
no influence upon these nervous phenomena. They may be complicated
with severe pulmonary lesions, of which M. Berger has collected two
examples. These nervous accidents resemble those described by Trous-
seau in cholera and dysentery, and they are always preceded or accom-
panied by a serious general condition of the system, to which Malgaigne
has given the name of hernial cholera. M. Berger, a report of whose
essay on the subject, was presented to the Société de Chirurgie, by M.
T. Anger, concludes that the existence of these nervous symptoms
indicates a very tight constriction which requires immediate liberation.—
Bull. Gen. de Therap., October 30.

INFLUENCE OF COLOURED LIGHT ON MANIACS.

DR. TAGUET, Physician to the Asylum of Ville-Evrard, has experi-
mentally investigated the sensational statements of Dr. Ponza as to the
influence of coloured light in subduing the violence of mania. In a room
whose windows were of blue glass, and whose walls were painted the
same colour, a patient labouring under strong maniacal excitement was
placed for three hours. The light seemed to fatigue him as he kept his
eyelids closed during most of the time, but no calming tendency was
apparent. For several days the experiment was continued, but with
negative results. The second case was one of acute mania with refusal
to take food, &c.; there was no improvement. A number of others were
tried, including some hysterical patients, but in all cases the light seemed
to weary rather than soothe them. The Director-General of the Asylum
at Moscow has given a full trial to the new treatment, but without the
slightest result.—*Annales Médico-Psychologiques.*   S. W.

TREATMENT OF ANTHRAX.

M. J. GUÉRIN, has laid before the Academy of Medicine his views on the
pathology and treatment of anthrax. He believes it to be the result of a
general low state of health, or of a foregoing diease; but he considers its
danger consists principally in the fact of a septicemic change in the matter
constituting the core. The septic properties generated may become a
poison, infecting the whole system. The indications, therefore, are to
neutralise the septic decomposition, and also to hinder its dissemination.
He applies over the inflamed part a large blister, with a hole in the
centre, to allow of topical antiseptics. The tumour, he believes, will soon
become benign and inert, and the enucleation of the core, if it does take
place, will not require the aid of the bistoury. A solution of nitrate of
silver is to be applied to any excavation that may form to prevent the
absorption of any altered fluids. He would have recourse to the mode of
treatment in phlegmonous erysipelas or inflammation arising from dissec-
tion-wounds, &c.—*Montpellier Medical.*   S. W.

PART I.

ORIGINAL COMMUNICATIONS.

ART. V.—*Anæsthesia and Amaurosis as effects of Brain-disease.*[a]
By CHARLES ÉDOUARD BROWN-SÉQUARD, M.D., Paris ;
F.R.C.P., London; F.R.S.

ANÆSTHESIA.

MR. PRESIDENT AND GENTLEMEN,—In the preceding lecture
I spoke of some parts of the brain which are considered as the
centres where sensitive impressions are perceived.   I have again
to speak of them before I come to the subject of amaurosis.
There are many places which have been considered as centres of
perception of sensitive impressions, and I must say that there are
facts which are favourable to each of those views; and this very
circumstance that there are such facts can be shown to be decisive
against each of those views also.   Because if you find facts which
seem to show, for instance, that the optic thalamus is the centre of
perception of sensitive impressions, and if you find facts showing
also that the posterior lobes of the brain are the centres of percep-
tion of sensitive impressions, the second set of facts clearly testifies
against the first supposition, and the first set is quite in opposition
to the second supposition.   There is an argument therefore against
these views in the very fact that there are cases supporting each of
them.

[a] A Discourse delivered before the President and Fellows of the King and Queen's
College of Physicians, Saturday, November 25, 1876.

Those parts of the brain which have been considered as centres of perception of sensitive impressions are, as I have said, very numerous. There was a time when physiologists discussed the question whether the medulla oblongata or the pons Varolii were not the only centres of perception of sensitive impressions. I will not go into an examination of the facts supporting these views, as I have no time to do so, but certainly there are experimental facts which seem to be conclusive in favour of them, while, on the other hand, there seem as certainly to be other facts which are in opposition to them.

Recently, through the influence exerted by medical practitioners on our knowledge of physiology, other views have been put forward. A most interesting fact has been ascertained first by Ludwig Türck, and afterwards more clearly and positively by my friend, Professor Charcot. That fact, in a clinical point of view, is extremely important; and there is no question that in most cases of disease in a certain part, which I will point out in an instant, there is anæsthesia. Professor Charcot considers that part not as a centre, but merely as a place of passage of conductors. The diagram before you shows, by red lines, the fibres which are supposed to be the conductors of sensitive impressions. They constitute the posterior part of the internal capsule, and that part of the corona radiata which goes to the posterior lobes. When that part of the internal capsule is diseased, there is a loss of sensibility on the opposite side of the body in certainly eight cases out of ten. But the two cases making the exception are, I must say, perfectly sufficient to show that the fibres do not possess the function attributed to them, and to show also that there must be another cause than a mere loss of function of the part diseased to make anæsthesia appear when it does appear. In that part of the internal capsule, as I have said, disease will generally produce anæsthesia in the opposite side of the body. But we cannot consider those fibres as being what these clinical facts seem to prove, not simply because, as I have said, there are cases in which they have been destroyed without anæsthesia, but also because the part of the brain to which they go—that is, the posterior lobe—is very frequently indeed diseased and even destroyed without anæsthesia.

If those fibres were the only means of communication between the sensitive parts of the body and the part of the brain where the perception of sensations takes place, the destruction of those fibres

would *always* produce anæsthesia; and also that part of the brain to which they go—namely, the cells and gray matter which they reach in the posterior cerebral lobe, could not be destroyed without the appearance of anæsthesia. Well, there are many, many cases indeed, nine out of ten—perhaps more, nineteen out of twenty— cases of considerable disease of the posterior lobe without anæsthesia. Therefore, with those two arguments, we find it impossible to admit that those fibres are what they have been considered to be—*i.e.*, the only channels of communication between the sensitive parts of the body and the central organ of perception of sensitive impressions.

Many other views, as I have already told you, have been put forward as regards the seat of the centre of sensitive impressions in the brain. With respect to some of them it is almost useless to speak. One of these, recently propounded, places the sensitive centre in the hippocampus major and the neighbouring convolution. The only ground for that supposition is an experiment consisting in the passage of a wire through the brain from behind, and evidently destroying, besides the hippocampus major, a good deal of brain tissue. The result obtained was anæsthesia in the opposite side of the body. But it is strange that the author of that experiment, who is a talented physiologist, seems not to have paid attention to the impossibility of destroying the hippocampus major without destroying also at least a number of those fibres of the internal capsule which are known to have the peculiar property, when they have been irritated by section, of producing anæsthesia. I shall therefore leave that view aside.

Another of these views is that the optic thalamus has been considered by a great many physiologists as a centre for the sensitive impressions coming from various parts of the body; and indeed Dr. Luys, an anatomist of great merit, but who is somewhat carried away by enthusiasm—I was about to say imagination, but perhaps it is more polite to say enthusiasm—has the idea that in the optic thalamus a centre exists, not only for the sensitive impressions that come from the skin and other organs of the trunk and limbs, but also for those that come from the nerves of vision, hearing, and smell. This hypothesis has, I must say, the great advantage of being a very comprehensive one, so that it is easy to find facts to prove or to disprove its correctness.

The supposition that all the sensory and all the sensitive fibres of the body have their centre in the optic thalamus

has a very good ground in appearance, if you only take a certain number of facts. But if any one of you, or all of you together, amused yourselves by putting forward any hypothesis whatever as regards the function of this or that part of the brain, you would find facts in favour of your views. There lies the danger. Indeed it is a great pity that physiologists and pathologists who have indulged in hypotheses have had a tendency to consider only facts favourable to their views. There certainly are facts favourable to the hypothesis in question; and the pathology of the optic thalamus is certainly most remarkable. There are cases of disease absolutely limited to one optic thalamus, with loss of vision, loss of hearing, loss of taste, loss of smell, and loss also of the tactile and general sensibility of the skin and all other parts on the opposite side of the body.

These cases are certainly extremely remarkable; and if I had time to speak of the optic thalamus I would show that, not only in respect of the power of producing loss of action of the various senses and loss of sensibility in the skin and elsewhere, but also in a great many other ways, that organ resembles the trigeminal nerve—a section of which may cause, with facial anæsthesia, a loss of vision, hearing, taste, and smell; and that therefore, if we were to conclude, as has been done, from that number of facts in favour of Dr. Luys' views, we should also have to conclude from a number of certain other facts that the trigeminal nerve is the centre of perception for the senses of sight, hearing, smell, taste, and of the sensations of touch, pain, &c., in the face and head. There are the same appearances of proof as regards that nerve as there are as regards the optic thalamus; and it is certain that when you have studied comparatively all that relates to disease of that nerve, and all that relates to disease of the optic thalamus, you will find that similar properties belong to those parts—even indeed so far as this, that an ulceration of the cornea may be caused by either of them, with this difference, however, that the effects of disease of the optic thalamus occur on the opposite side, and those of lesions of the trigeminal nerve take place on the side of the lesions. But it is in virtue of special properties they possess that these parts act as they do when irritated by disease, and properties are not functions.

There is in those parts the property or power of producing sometimes, under irritation, something similar to the arrest of the heart's action when the par vagum is irritated—an arresting or inhibitory,

or, if you prefer it, a suspending or stopping influence on the eye, the ear, and several other parts, so that the action of those organs cease to be performed. As regards the trigeminal nerve, there are many cases indeed showing that an irritation of any of its branches, but chiefly the supra and infra-orbitalis, can arrest the activity of the organ of vision—that is, destroy the sense of sight—can arrest the activity of the organ of hearing, or of any other sense, and also destroy the sensibility, not simply in those parts to which the branch which is injured sends ramifications, but in all the parts to which the other branches go. This is nothing but what we see through the influence of irritation of other parts of the nervous system. Under irritation the bowels, for instance, can produce an inhibitory influence on the senses, an inhibitory influence on the general sensibility, and so on. So that what relates to the optic thalamus is much easier explained when it is admitted that under irritation the part can act upon either the retina or the auditory nerve or other nerves, and arrest, inhibit, or suspend the activity of the cells in the organs that lose their functions.

I will not now say much more as regards the centres of sensitive impressions, since my friend, Professor Vulpian, has found that in cases of disease of the brain with anæsthesia, it is very frequently quite sufficient to pass a strong galvanic current over the anæsthetic parts to cause the anæsthesia to vanish. It seems clear that anæsthesia, in cases of disease of the brain, depends on something else than the loss of function of pretended centres and pretended conductors inside the brain, and is due only—as I have tried to show elsewhere, and will try to show at the end of the next lecture if I have time—to what I have described so many times as an arresting, inhibitory, or suspending influence exerted on distant parts by an irritation that starts from the place where the disease exists.

There are facts, however, which, although I am in a hurry to progress, I cannot help mentioning. It was not without some feeling akin to that felt by a parent at the loss of offspring that I sacrificed completely—and I did so without any hesitation, however, in the lecture that I delivered here the other day—the view which I had myself tried to establish, and the correctness of which I had succeeded, as I thought, in proving—that sensitive impressions are conveyed from the right side of the body, so as to pass to the left side of the spinal cord, and to go to the brain along that left side. I now believe that I made a mistake in that, as I told you. But

the questions remain—by what mechanism are sensitive impressions conveyed to the perceptive centre, and where is the perceptive centre? and also how it is that the section of the lateral half of the spinal cord in man, as well as in animals, will destroy the sensibility of the opposite side of the body. I cannot dwell at length on these points; but, about the last, what I believe can be established is, that each half of the spinal cord conveys sensitive impressions to the brain from each of the two sides of the body. To be more specific, I will say that in the right half of the cord there are conductors that transmit sensitive impressions coming from the two sides of the body, and *vice versâ* for the other half of the cord. If the section of a lateral half of the cord produces anæsthesia, it is just in the same way as a mere prick of the posterior column will produce anæsthesia on the opposite side. If you prick the posterior column you do not destroy the conductors—you produce irritation in a very limited part. Only a few fibres are irritated, still you have exactly the same effect and result as if you had cut half of the spinal cord. You certainly cannot conclude from the effect of the prick of the posterior column that you have destroyed all the conductors, or indeed that you have destroyed more than a very minute number of the conductors of sensitive impressions to the brain. Therefore this experiment shows that there must be something else than the mere section of the conductors to produce anæsthesia in those cases.

But on what ground am I entitled to say that each half of the spinal cord receives conductors from the two sides of the body? On this—I had already, in a number of experiments years ago, found some anomalies in appearance refractory to what I tried to establish. I divided, as I thought, one lateral half of the spinal cord, and still sensibility appeared not to be destroyed on the opposite side of the body. If I made a dissection of the animal then, I generally found that a small amount of the gray matter of the cord had been left, and I concluded that, through a number of conductors left in that remaining small part of gray matter, the transmission of sensitive impressions was able to continue. And as, when you divide the posterior column on one side—and you do that when you divide half of the cord—there is a cause of a great increase of sensibility in the paralysed parts of the body, it seemed to me that the sensibility had remained and seemed to be great, when in reality it was exceedingly small, being, through that cause of increase, apparently

tenfold what in reality it was. And that shows that unfortunately when we are bent upon a view with the desire of establishing it, we may go much beyond what is really taught by facts, and sometimes beyond what is rational. I certainly then committed a mistake, and I am very glad to say, frankly and openly, to what extent I made it.

There is another reason to show that one lateral half of the cord is sufficient to convey impressions coming from the two sides of the body. When I say another reason, I mean the same fact, but observed in man. There are cases in which, notwithstanding an alteration of a great part of a lateral half of the cord, no anæsthesia took place. There are cases—I mentioned some such in the last lecture—of destruction of one of the lateral halves of the medulla oblongata, and in which anæsthesia was caused, not on the opposite side, but on the corresponding side. And as we cannot admit that in some men there is a decussation of the conductors of sensitive impressions, and in others not—as we cannot admit the existence of such a monstrosity, we must consider these facts as quite decisive in showing that it is not through an alteration of conductors, but owing to some other cause, that anæsthesia appears either on the opposite side or on the same side of a lesion in the cord or the medulla oblongata. I should like to say more of anæsthesia, but time presses, and I will pass now to the subject of—

AMAUROSIS AS AN EFFECT OF BRAIN-DISEASE.

Amaurosis, as you well know, can exist in one eye or the other; and if I only had time, I think that, by facts relating to amaurosis, I might prove, much more easily than I have been able to do by facts relating to anæsthesia, that it is not owing to disease of the brain destroying either centres or fibres serving as conductors that symptoms of loss of the functions of the cerebral organs appear. It can be abundantly proved that it is not owing to the destruction of pretended centres or conductors that amaurosis appears in cases of brain-disease, even when that lesion is not entirely or only partly in the cerebral lobes, but when it exists in the optic tract on one side, although that tract seems to be simply a continuation of the optic nerve. About that I must say at once that the optic nerve ought to be considered as being a bundle of fibres establishing a communication between two centres, and therefore that it ought to be looked upon in the same way as those fibres within the

brain which unite cells with other cells. It is therefore, if I am right in that respect, to be considered as a part of the brain—and indeed the retina, the optic tract, the optic nerve, and the chiasma, according to my view, would each and all form parts of the brain. So it is, you might say, as regards all the other nerves of sensibility, and perhaps also as regards the nerves of motion. It may be, after all, that the distinction which is so absolute in the minds of medical men, and was so in my own mind for so long, between the peripheral and the central parts of the nervous system, is a false distinction, and that nerves are merely connecting fibres between cells in the nervous centres and cells and other nervous elements resembling cells in the periphery.

However, I will leave aside what relates to the other parts of the nervous system, and consider only what appertains to the optic nerve and the optic tract, besides the chiasma and the retina.

The view now well known as Wollaston's theory, and which had been put forward long ago by perhaps one of the greatest men that ever lived in this country, Newton, has been—particularly within the last five or six years—considered perfectly sufficient to explain many of the phenomena of vision, physiological and pathological, and especially hemiopia. There were difficulties, however, about it which struck a good many persons; and Professor Charcot has put forward a view which I am afraid I will not have time to mention, but which certainly will be abandoned very soon by that eminent physiologist, if he has not already abandoned it, as it cannot stand being put face to face with facts.

The view of Wollaston consists in this. [The lecturer referred to plates.] If you take these parts as representing the optic tracts, you see that they meet at the chiasma, and that each of them is composed of two parts—an outer and an inner one. The outer one, after reaching the chiasma, is reflected on itself, and goes to the outer part of the eye on the same side. The inner parts of the optic tracts cross each other in the chiasma, so that the one coming from the right side of the brain goes to the inner half of the left eye, and the one from the left side of the brain to the inner half of the right eye; so that the right tract is distributed to the outer half of the right eye, and to the inner half of the left eye, those two halves of the eyes acting together for the sight of the same half of objects. It is quite easy to understand then that one of the optic tracts serves us to see one half of an object, while the other one serves us

to see the other half. Up to this moment it is not quite decided what the real structure of the chiasma is; but there are points with respect to which most anatomists agree. For instance, it is almost universally admitted that there are fibres going from one retina to the other, passing in front of the chiasma (a fact which, by the way, shows that the retinæ are nervous centres), and also others coming from the base of the brain at the back of the chiasma, and going from one optic band to the other. There are therefore two sets of fibres distinct from those of the optic nerve— one set connecting the two retinæ, the other being a true com- missure between the two cerebral hemispheres.

According to Wollaston's theory, if there is in the right optic band or tract a lesion destroying all its fibres, there ought necessarily to be loss of vision in the internal half of the left eye, and in the external half of the right eye. When such a thing exists—and there are such cases of disease of the optic band on one side having produced that hemiopia, as represented here by this white surface on the two retinæ—it cer- tainly is perfectly in agreement with the view of Wollaston. There are cases favourable to this view; I had one myself, and during the life of the patient predicted that the autopsy would show disease of the tubercula quadrigemina on one side. The autopsy was made by my able friend, Dr. J. Hughlings Jackson, and the view I had held was found correct; but you will find by-and-by that there are a great many facts in opposition to this view. In the case you see represented on the diagram there is only hemiopia of one eye. There are cases favourable to this, and you can easily conceive how it may be so. Supposing there is disease in only the external part or half of the optic band, there will be, according to the theory, loss of sight in the part of the eye on the same side which corresponds with that external half of the optic band. Such cases, of course, are quite favourable to the theory.

But now here is the difficulty:—You see represented here a case in which the two outer halves of the retina are diseased. To explain this there must be a disease that strikes at the same time either the two outer parts of the optic bands or the two outer parts of the chiasma. I saw such a case—I must say that I did not diagnose it; on the contrary, I declared that I did not understand at all what the case was. My colleague, Dr. Knapp, of Germany, now a most eminent oculist of New York,

who attended the patient, was also quite uncertain about the diagnosis. After the patient died it was found that the case was indeed a very simple one, as you may see in the details of the case published by Dr. Knapp in my American Journal. There was disease of the blood-vessels on the two sides of the chiasma, and the external part of each optic band having been thus injured, it was very natural, according to Wollaston's theory, that there should be such a peculiar alteration of sight. Therefore this kind of case may be in harmony with the views of Wollaston.

But there are such cases in which the autopsy has shown nothing but a disease in some part of the brain, and nothing whatever wrong with the optic bands, and these cases are certainly quite against Wollaston's theory. There are cases of disease, as I will show in a moment, of one optic band, in which amaurosis of one eye only has been produced, the two halves of one retina being therefore affected. Supposing the left optic band to be diseased, the two halves of either the left or the right eye may lose the power of sight. I have a large number of facts showing disease of one half of the brain, or located in the tubercula quadrigemina, in the corpora geniculata, or in the optic band on one side, having caused loss of vision of the two halves of the retina either on the same or on the opposite side. This is a decisive fact against Wollaston's theory. It is quite certain that in a case of that kind it is what I showed you first which should always exist—that is, hemiopia in one or the other eye, or in both. Therefore if we had but one single such fact showing that the two halves of one eye had lost their power of sight from disease of the optic band on the same or on the opposite side, it would be decisive against the theory. But I have a great many such cases, and there is no question that this is absolutely destructive to the admission of the views of Wollaston. There are many cases in which the optic band on the right side was destroyed, and yet, instead of loss of sight in the outer half of the right eye and in the inner half of the left eye being produced, the loss of sight was in both halves of either the left or the right eye—an effect which, according to Wollaston's theory, should never exist. It has been suggested—and this was said to me by Dr. Knapp himself, who, I repeat, is a very able man indeed—that perhaps during the life of the patient no care had been taken to ascertain whether or not there was only hemiopia in the eye which was considered as completely blind. That mistake may have been committed;

but, at the same time, in the presence of existing facts, it is impossible to deny that very frequently at least there has been loss of sight in the two halves of the one eye, sometimes on the same side and sometimes on the opposite side, from disease in the optic band, or in the parts in which that optic band throws itself.

My own experience on that point does not include autopsies, but is grounded upon cases in which many other symptoms led to a diagnosis, so that it leaves no doubt that there are a number of cases in which one eye has lost its power from disease located in parts of one half of the base of the brain. The symptoms are so clear that there is certainly no question that the eye on the same side or on the opposite side can be rendered entirely blind, or at any rate that the two halves of it can lose the greater part of their power of sight from disease located in one half of the base of the brain and behind the chiasma—that is, behind the point of decussation of the optic tracts.

But there is more—and this is certainly one of the most decisive proofs, though I will mention presently one still more positive. Here is a drawing, to show the loss of sight in both eyes, which has been found in a great many cases in which disease of the base of the brain was limited to one side only; so that disease limited to the base of the brain on one side can produce amaurosis in the two halves of the eye on the same side, amaurosis in the two halves of the eye on the opposite side, and also amaurosis in the two halves of both eyes.

But this is not all. There are cases, of which I have collected a number, in which the optic band has been destroyed without any amaurosis at all. How, then, is Wollaston's theory to subsist? What ground has it after such a fact showing that when one of the optic bands has been destroyed, sight can continue on the two sides? I have a number of cases in which the alteration had taken place where the optic bands form the chiasma. Here is a case observed and recorded by Dr. Nélaton, of Paris—certainly a very good observer—in which the greater part of the chiasma, at its centre especially, had been completely destroyed. It was a gelatinous mass having no more fibres, and the fibres still existing in the chiasma belonged to the outside set. In that case there was no trace of amaurosis at all—no diminution of sight. The other cases which I have include a remarkable one published by Dr. Sawyer, of London, in *The Lancet,*

in which there was considerable pressure on the chiasma without
any trace of amaurosis. One of them is a case in which the autopsy
was made, I think, by a gentleman who is present in this room,
one of the ablest medical men of this city, Dr. Robert M'Donnell,
who at any rate had care of the case, and in it there had been a
slowly developing morbid change in the chiasma, and going on for
a long time, and then all on a sudden amaurosis came on—just as
we see paralysis, anæsthesia, convulsions, and other symptoms of
brain-disease appearing suddenly from a lesion which had existed
for a long time and been increased more or less gradually. There
are many cases, I must say, in which amaurosis from brain disease
has appeared in that way all on a sudden, although the lesion
from which it arose had been going on for a long time. There are
besides a great many cases in which amaurosis appears and dis-
appears, although the lesion continuously exists in those parts
which are considered as having their fibres as conductors serving
the purposes of sight, or on those parts which are considered as
centres for the perception of sight inside the brain.

There are therefore a great many facts which prevent our admit-
ting the correctness of Wollaston's theory. And indeed we must
admit that one half of the brain is sufficient for vision on the two
sides. We must admit that when amaurosis appears from disease
located in one half of the brain, or in one optic band, or in one part
of the chiasma, it results not from the destruction of the conductors
or of the centres, but from action exerted on some distant parts
by those where the disease is, and from the irritation that exists
where the disease is found. Experiments on animals, and also facts
which have been observed in man, show how variously and how
frequently there are alterations of vision, consisting in either mere
hemiopia or complete amaurosis caused by reflex influences. Take
the cases, for instance, in which the corpus restiforme or the
medulla oblongata is pricked. In these cases, as I have found,
very frequently amaurosis appears in the eye on the same side.
We have no reason to consider that the corpus restiforme contains
conductors or nerve-centres of vision. Take cases of disease of the
cerebellum, and in these you will find amaurosis appearing either
in both eyes, from an injury limited to half of the cerebellum, or in
the eye on the same side of, or in that on the opposite side to the
injury, and this through irritation, and often through the same
mechanism that makes amaurosis appear when there is irritation in
the bowels. Take also the cases of amaurosis due to dental irrita-

tion. A tooth is diseased; great irritation results, and sight is lost on the same side. The tooth is extracted; the cause of irritation is removed; sight re-appears. Unfortunately we have not the key of the dentist to take away those causes of irritation which, in the brain, engender amaurosis; but I must say I am sanguine enough to hope that, with our new knowledge of the power of inhibition, we shall, before very long, be able to do what the dentist does in that case, and it may be so not only as regards amaurosis, but also as regards paralysis, anæsthesia, and all other symptoms of brain-disease resulting from loss of activity in nerve cells. If we knew only where to strike in order to take away the cause of the arrest or inhibition which exists in those cases, we should then be able to do what the dentist does, and with the effect I have mentioned.

I have now to say a few words on what have been considered as the centres inside the brain, and the facts I shall mention are exceedingly interesting. If you look at the two parts that compose the optic band—the external and the internal—you will find that a great deal of the internal goes to the internal corpus geniculatum on the same side, and of the external to the external corpus geniculatum also on the same side. Now, disease has been found in some cases occupying those two corpora geniculata, and in those cases the loss of sight was altogether in the opposite side, which is, of course, against Wollaston's views. But what if we consider the corpora geniculata as centres of perception of visual sensations? There are cases recorded in which they have been destroyed on the one side without any marked diminution of the power of sight. Now, facts of that kind show conclusively that one side of the base of the brain is quite sufficient for the function of the two eyes. If we examine the facts and views that have been put forward with regard to the tubercula quadrigemina, we find that they also have been regarded as centres, and yet that they have been destroyed without any considerable diminution of the power of sight. Generally—almost always I should say—there is a diminution of sight in those cases; but even one case to the contrary is enough to show that the view of Wollaston must be thrown aside.

The other centres which have been considered as those of vision are many—one of them is the optic thalamus, which must be put out of the question. Facts abound against that view. In two cases of destruction by the same process—namely, inflammatory softening of one and the same optic thalamus, one patient altogether lost the sight of the eye on the opposite side, and the

other lost altogether the sight of the eye on the same side. That, of course, shows that we cannot consider the optic thalami as centres, as otherwise we would have to deny that there is some constant organisation to serve the same functions in all men. There are a great many cases in which the optic thalamus has been destroyed on the one side without any alteration of sight on the other, and still more in which both optic thalami have been destroyed without the least alteration of vision; so that we must put aside the supposition that those cerebral ganglions are centres of vision.

. With regard to another alleged centre of vision, the experiments of Dr. Ferrier are interesting as far as they go, but they do not prove what he believes. He thinks that a part of the middle lobe very near the posterior lobe is the centre of vision, but he has himself seen that the destruction of that part on one side does not produce blindness. He had to destroy that part on both sides in order to produce amaurosis, and then it existed in the two eyes. Certainly his experiment is a very interesting one, but what does it show more than what I have shown to occur when the corpus restiforme is pricked? On doing that on one side I found loss of sight in one eye; and if I pricked both corpora restiformia, a loss of sight in the two eyes was sometimes produced. If we were to conclude, because the mere prick of that part of the medulla oblongata caused blindness, that any of the places pricked—any of the fibres injured—are centres of vision, we should have to admit that the centres of vision are almost everywhere in the system, because a great many nerves when injured, inflamed, or attacked with neuralgia, neuroma, &c., or, in other words, irritated, have been able to produce amaurosis by reflex action. Besides, the cerebellum itself should be considered as the centre of vision, as a lesion of that organ in man so frequently causes amaurosis. We can therefore only consider the interesting facts of Dr. Ferrier as showing that certain parts of the brain have the power, when injured, of causing loss of sight, most likely in the same way as the tickling of the soles of the feet has the power of producing the contraction of certain muscles of the face. There is no conclusion beyond that to be drawn from those facts.

As regards the other places serving as centres, Professor Charcot has propounded the view that the fibres of the optic nerves that do not decussate in the chiasma make their decussation in the base of the brain, and that these two sets of fibres

meet in some part of the brain, and he explains in that way the loss of sight in the opposite eye which we find sometimes when disease exists in certain parts of one side of the brain. I will not discuss at length all the bearings of that view. I will merely say that there is no part of the brain which, being the seat of a lesion, always produces amaurosis, especially amaurosis of the opposite eye; and, on the other hand, that disease may exist anywhere without causing amaurosis. Besides, a lesion in one side of the brain can produce amaurosis either in one eye or in the other, or in both eyes. I add that there are a great many cases of alterations of either the anterior, the middle, or the posterior lobes destroying the power of sight, and many more cases of lesion in the same parts without amaurosis. I said a moment ago that there are many cases on record in which amaurosis from brain-disease shows itself, then disappears, many times in succession; and it is not rare to find cases in which amaurosis came by attacks like epilepsy, and which certainly have been caused by organic disease of the brain. The number of such cases in which autopsies have been made is now pretty large; and I may say that every kind of symptoms of brain-disease, consisting in loss of function of some part of the nervous centres, such as paralysis, anæsthesia, aphasia, amaurosis, deafness, and so on, can appear intermittently, although the disease producing these symptoms has been constantly persistent. They can appear by attacks just like epilepsy. In my next lecture I shall treat of APHASIA.

ART. VI.—*On the Management of the Bowels in Enteric Fever.*
  By THOMAS W. GRIMSHAW, M.A., M.D., Dubl.; Fellow and Censor of the King and Queen's College of Physicians; Physician to Steevens' and to Cork-street (Fever) Hospitals.

THE management of the bowels in enteric fever may seem rather a trivial subject to treat of before this Society, but I have been induced to direct attention to this department of the therapeutics of fever in consequence of the many serious mishaps which have come under my notice, owing, I believe, to a too slight appreciation of the importance of watching closely and continuously, and

carefully regulating the action of the bowels in this now, unfortunately, common disease in Dublin.

The question of the management of the bowels in enteric fever has been one upon which different opinions have prevailed among the highest authorities. Thus Dr. Todd says:—" Restrain diarrhœa and hæmorrhage in typhoid fever, and when you have fairly locked up the bowels, keep them so. Patients will go for four or six days, or even longer, without suffering inconvenience from this state of constipation." Other writers, Drs. Johnson, Gardiner, and Maclagan, recommend " laxatives."

Dr. Murchison says:—" When there is constipation at the commencement of the attack, it is well to commence the treatment with a small dose of castor-oil, or rhubarb in peppermint water;" and when the bowels are confined at a later stage, I am in the habit of prescribing, every second or third day, one or two teaspoonfuls of castor-oil, or a simple enema. But when constipation succeeds to severe diarrhœa, the best practice, I think, is to abstain from interfering for four or five days, and then only to prescribe a simple enema, or one teaspoonful of castor-oil." [a]

The practice of using purgatives in fevers generally, is considered by Dr. Hudson, who says:—" Among the worst cases admitted into hospital are those of patients who have been dosed with salts by themselves or their friends."

This sentence was written by Dr. Hudson in 1867, or now nearly ten years ago; it was repeated by him in the new edition of his work in 1868, and yet this dangerous practice of administering violent purgatives to patients at the commencement of fever is pursued not only by the patients and their friends, but even by some practitioners of medicine in Dublin and elsewhere, who ought to know better. I believe the injudicious use of astringents at the commencement of enteric fever is almost, if not quite, as injurious as the administration of purgatives. The administration of a violent purgative to a patient suffering from enteric fever in its early stage has this advantage to the patient over the use of an astringent—namely, that it effectually prevents his going about, and soon drives him to bed or to hospital, where he has at least some prospect of rest and quiet, and generally of more judicious treatment. The astringent treatment, on the other hand, has generally the effect of enabling the patient to go about a little longer, and thus more quickly use up his failing strength,

* Murchison. 2nd Edition. 1873.

gradually drifting him into a state of established disease, which is certain to be of long duration, terminating in tardy or imperfect convalescence, and not infrequently in death. The number of cases of enteric fever which have come under my notice where injury has been done by the injudicious use of astringent or purgative medicine, is almost incredible.

I do not propose to give details of cases here, but, as examples, I mention a few instances which have recently come under my notice.

CASE I.—A lad aged about sixteen, admitted to Steevens' Hospital, stated that "he had had a looseness of bowels; went to a doctor who *stopped* it; he never was bad till his bowels stopped." He then came to Steevens' Hospital, where he was found to have a swollen tympanitic abdomen, tender *all over*, dry brown tongue, the characteristic rash, and all the symptoms of severe enteric fever of about fourteen days' duration; his bowels had now been confined for a week. He was ordered one teaspoonful of castor-oil, with six minims of tincture of opium, and if action of the bowels did not take place in a few hours, a simple water enema was to be given. The enema was not required. The oil acted well, effectually relieving the tenderness and distension; the amount of fæcal matter passed was so large and so offensive, and followed by such considerable depression, that some doubt was entertained as to the ultimate result to the patient. He made a very slow recovery, the iliac tenderness remaining much longer than usual.

CASE II.—Another almost similar case was admitted to Corkstreet Hospital, which unfortunately terminated fatally.

CASE III.—A case was admitted to Steevens' Hospital of a man, aged thirty-four, who had dosed himself with salts and senna by the advice of a woman who said "he was bilious, and only wanted a physic." He got so bad after the physic that he sent for the dispensary doctor, who ordered him to hospital. When he was admitted he looked almost like a cholera patient; he was in a state of collapse; his temperature fell below normal; and in spite of all treatment but little rally took place, and he died, in my opinion, from the effect of "the physic."

CASE IV.—A young man, driver of a bread-cart, stated he felt unwell about a week before admission; went to Dr. ——, who said he had a gastric attack; he gave him a physic—salts and senna, he

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thought—which purged him. He went again to the same doctor; the next day he gave him another "physic," which "purged the life out of him." He then sent for the "dispensary doctor, who said he had the fever, and Dr. —— was killing him," and sent him to hospital. The poor fellow's account that "Dr. —— had purged the life out of him," was very nearly fulfilled to the letter. He became almost collapsed; the diarrhœa was with difficulty re-strained; stimulants and large doses of quinine saved him, but not before every prominence upon his body had shown a tendency to bed-sores, and a large and deep bed-sore had actually formed over the sacrum in spite of careful watching.

The foregoing are typical examples of classes of cases of which I have met considerable numbers.

Now it may be asked how is it possible, in the present state of medical knowledge, that such serious results should occur even in the hands of duly qualified medical men? I have no desire to be hard upon my professional brethren; but I must confess that I believe that in the majority of cases where violent purgatives or powerful astringents have been given to patients suffering from the early stages of enteric fever, the agents are prescribed and adminis-tered as a consequence of a mistaken diagnosis, or perhaps no diagnosis at all. It is by no means easy always to diagnose a case of enteric fever in its earliest stage; but no one should ever venture upon active treatment until the nature of the disease has been made out. The frequency with which a "looseness of the bowels" or a "gastric attack" has been treated simply *as such* by astringents in one case, or "a good clearing out" in the other, without any care-ful attempt having been made either to determine the cause of the looseness, or the nature of the gastric attack, is a disgrace to the profession of medicine. In many instances, this looseness of the bowels, or gastric attack, is the commencement of a serious attack of enteric fever, which, if carefully diagnosed and treated accord-ingly, might have terminated favourably.

We know that patients walk about at the commencement of and not infrequently during the greater portion of the course of an attack of enteric fever, and some persons have excused an error of diagnosis upon the ground that the patient came to their house to consult them, and had *only* a furred tongue, a quick pulse, and had been in the same state for a week. It may be the result of my peculiar training, but I must say I would assume that a patient with a quick,

weak pulse, furred tongue, loss of appetite, and who had been in the same state for a week, had enteric fever, unless there was good cause to believe to the contrary. If I find, on inquiry, that he had been chilly, had vomiting, pains, and either constipation or diarrhœa, and irritation of the urinary organs, I would consider my diagnosis almost certain, even before I had an opportunity of testing the temperature and examining for an eruption, or ascertaining the nature of the discharge from the bowels. Careful observation has convinced me that some practitioners stop short after the discovery of the quick pulse and furred tongue, and only inquire for a headache or for the state of the bowels. If the bowels are confined, the inevitable purgative follows, which is only regulated in intensity by the length of the constipation and the habit of the patient. If the bowels are too free, then as certainly follows the astringent. In a few days more the patient is on the brink of the grave from enteric fever. In other instances the practitioner explains to the patient or his friends that he has " only got gastric fever." Now this is a course which should be scorned by every educated physician. He should, in every case, state, emphatically and distinctly, that the disease is *typhoid fever*, which is the term the public are familiar with as the title of this very severe and treacherous form of disease. I believe it is scarcely excusable for a physician to mistake enteric fever for merely gastro-intestinal catarrh, inasmuch as in a large majority of instances a catarrhal condition of the mucous membrane occurs at the commencement of enteric fever, and should be taken as pointing to an impending attack of that disease.

Now, having pointed out the errors of treatment which so often follow an erroneous or incomplete diagnosis, there still remains, in my opinion, another source of danger, and it is this—that the diagnosis having been made correctly, some persons deliberately and advisedly administer powerful purgatives and astringents for the cure of the disease; in the first case with the view of eliminating the specific poison of the disease, and in the second, with the intention of sustaining the patient's strength.

It appears to me to be extremely irrational to expect that in a case where the bowels are already free, possibly too free, that any additional eliminative power will be gained by the administration of a stimulant to an already over-acting organ; and, further, we should not forget that we have an irritated, inflamed, and probably ulcerated intestine which is sadly in want of rest to enable it to return to health. The prescriber of astringents, on the other hand, also

forgets the inflamed and ulcerated intestines, and that by the use of astringents he is retaining irritating and decomposing matters in contact with the ulcers, thus increasing their irritation, and promoting the tendency to septic poisoning and deep ulceration, which are the great dangers in enteric fever.

Now having so severely criticised the practice of others, I may be fairly expected to mention what practice I pursue myself, and consequently what I recommend.   It is difficult to treat of *one* condition of a disease without considering the treatment of other concurrent states.

In the first place, I consider it essential to the fair progress of a case of enteric fever that the bowels should be more frequently moved than in health, and that the motions should be plentiful. I consider that the bowels may be moved with advantage to the patient four times in twenty-four hours, and should never be allowed to remain confined for more than forty-eight hours, and not so long if any symptoms of distension or pain set in.

The measures I take to promote these objects, in many instances, are confined to mere regulation of diet—and for many valuable hints upon this point I am indebted to my friend and former colleague, Dr. H. Kennedy, who has published an interesting paper on this subject in *The Practitioner*. Thus, if the bowels are moved more than the required number of times without the diarrhœa being a severe symptom, I find that feeding the patient on boiled milk alone will be sufficient—if not, the addition of saccharated lime-water will probably prove effective. If these remedies do not keep the diarrhœa within reasonable bounds—say under six motions in the twenty-four hours—I employ sulphuric acid in the proportion of 33 to an ℥8 mixture, one ounce to be taken every three hours. This I generally give as an addition to a mixture containing quinine, which I almost invariably employ in large doses in treating enteric fever.   Should this fail I add morphia or tincture of opium to the mixture in small quantities. I find that in cases where the boiled milk treatment is employed early little if any astringent medicines are required.   I never employ beef-tea when there is a tendency to excessive freedom of the bowels. In cases of extreme diarrhœa I have employed the lead and opium pills of the Pharmacopœia in 4 gr. doses every fourth hour with great benefit. At the same time I employ linseed poultices over the abdomen, and stupes of turpentine or mustard where pain or tenderness is much complained of. The treatment of constipation is a more easy affair. I may say for

this purpose I employ a single drug—namely, castor-oil, and usually muzzle it with opium. I seldom give more than a teaspoonful for a dose, and in many cases but half that amount. In the early stage of the disease, when I find the bowels have been confined for some days before the patient came under treatment, I at once give a dose of castor-oil. This not only benefits the patient, but in a doubtful case assists the diagnosis by often producing a characteristic evacuation. Great caution must always be observed in giving meat in early convalescence, as it is likely to produce diarrhœa. I prefer here to begin with chicken-broth, then chicken, and lastly mutton. If a rise in temperature occurs after a change of diet, diarrhœa may be expected, and should not be waited for; the meat should be at once discontinued, and the milk resumed. In cases of hæmorrhage I have found ergot the most useful remedy, and so far have never lost a case of enteric fever by hæmorrhage.

I believe the main point to be attended to in the management of the bowels in enteric fever is to keep them *free*, but *not too free*, and to avoid, as much as possible, purgatives or astringents. My reason for occupying the time of the Society by a paper of so elementary a character is that I have met with many serious consequences from the neglect of what appear to me to be obvious rules, and I feel that I can speak with some little confidence upon this subject after a thirteen years' experience of a large fever hospital, in addition to a nine years' experience of the fever wards of Steevens' Hospital.

ART. VII.—*Two Cases of Fracture of the Skull.* By EDW. WOLFENDEN COLLINS, M.D., F.R.C.S.I.; Surgeon to Jervis-street Hospital.

I DESIRE to bring under your notice two cases of fracture of the skull implicating the temporal bone, and each presenting some features of interest.

CASE I.—In July last a little girl, seventeen months old, was admitted into Jervis-street Hospital under my care, having fallen from her mother's arms, out of a window on the second story, into the area underneath. She was taken up insensible. When I saw her shortly afterwards she presented the phenomena of concussion

* Read before the Dublin Biological Club, Tuesday, December 19, 1876.

of the brain—collapse, imperfect insensibility from which she could be roused, vomiting, &c. On examination of the head I detected a well-marked depression over the posterior part of the left temporal bone. The line of depression was curved from above downwards and backwards, and accurately corresponded with the position of the squamo-parietal suture. The depression was lost inferiorly behind the mastoid region, whence the separation or fracture may have extended inwards to the base of the skull, though there were no phenomena indicating this event. The head was shaved and ice kept constantly applied. Diaphoretic medicine was administered, and subsequently purgatives when needed. In the absence of the symptoms of compression operative interference was not resorted to. The symptoms of concussion passed off without severe reaction. No further intracranial complications arose, and ere a month had elapsed the child was allowed to leave the hospital apparently in good health, the depression still existing.

The early age at which the accident occurred, the position of the depression, and its well-marked curvature, led me to the conclusion that in this instance diastasis of the bones at the posterior part of the squamo-parietal suture had occurred.

Mr. Prescott Hewitt[a] appears to have met with a very similar case. Remarking that "a separation of sutures without a fracture is a very rare form of injury," he adds :—" I have observed it but once. It occurred in the back part of the squamo-parietal suture. The temporal having been slightly separated from the parietal, and driven upwards, these bones presented at first sight the appearance of a fracture with depression." Analysis of cases of diastasis of the sutures shows the form of which I have recorded an instance to be among the rarest, the bones entering into the formation of the coronal, lambdoid, and sagital sutures, being far more commonly implicated, and in the order I have given. That diastasis existed along the posterior part of the left squamo-parietal suture I have no doubt, but I have some difficulty in believing that it was unattended with further fracture. Surgical writers are well agreed that these diastases usually co-exist with extensive fracture of the base, and are most dangerous in their nature. This was Mr. Pott's experience:[b]—" It sometimes happens," he says, " more particularly in young subjects, that we find a suture considerably disjoined, in

[a] *Cf.* Art. in Holmes' Surgery. Vol. II., p. 286.
[b] *Cf.* Vol. I., p. 184.

which circumstance I do not remember to have seen one single instance of recovery."

Case II.—During the early part of the same month, a boy, eight years old, was pitched, head-foremost, from the back of an unruly horse which he was riding to its stable. He was picked up senseless, and bleeding from the left ear. I saw him at seven in the evening, shortly after his admission into Jervis-street Hospital, whither his friends had carried him. His condition was one of partial insensibility. He would answer rationally when loudly addressed, and then relapse into a state resembling sleep. He vomited at times, particularly when moved or roused. Both pupils were equally dilated and responded to light. His temp. was 98° F.; pulse 70, weak and irregular; respirations 24 per minute. The bleeding from the left ear had almost ceased, and was being replaced by a clear watery discharge, which kept welling up in the meatus, and overflowed from the concha only when the patient was inclined towards the injured side.

I concluded that the boy had sustained concussion of the brain along with a fracture running through the petrous portion of the temporal bone, which probably implicated the internal auditory canal and its membranes, thus opening the subarachnoid space. That the watery discharge coming from the ear was the cerebro-spinal fluid I had no doubt, owing to its copiousness and almost immediate occurrence after the accident. The slight bleeding from the ear which preceded it, and the peculiar manner in which the discharge continued to well up to the top of the meatus, after its removal by a sponge, pointed less surely in the same direction. A cathartic bolus was administered, and ice kept constantly applied to the shaven head of the boy during the night, and subsequently throughout his illness.

Upon the following morning his temp. was subnormal, 97·8° F., while his pulse had risen 20 beats in the minute. A slight degree of facial paralysis existed on the affected side. This continued, but did not subsequently increase. He had completely regained his consciousness. He had no headache. Pressure over the left mastoid process, however, was attended with suffering. The limpid discharge from the ear had became more copious, and now overflowed in drops, even when the boy lay upon the unaffected side. For purposes of examination I collected in a short time somewhat more than an ounce. As the boy had passed no urine

since the accident I relieved his bladder by a gum-elastic catheter, preserving the urine also for examination. This retention continued during this and the following day, and again recurred during the later period of his illness. The bowels during the same period remained obstinately constipated. To obviate this condition large doses of calomel and jalap in bolus, and enemata of Epsom salts were employed without effect. Upon the third day two large doses of croton-oil were more successful. The constipation subsequently recurred when intracranial inflammation ensued.

Occasional vomiting, obstinate constipation, and urinary retention were the unfavourable signs the boy manifested during forty-eight hours after the accident. He complained of no headache, and slept quietly during the greater part of this time. His temp. remained somewhat lower than 98° F. His pulse varied between 70 and 90 in the minute. By the morning of the third day, however, intracranial inflammation had set in. The pulse had risen to 112; the temp. to 100° F. The cheeks were flushed. The temp. continued to rise till the morning of the fifth day, when it attained its maximum of 101·6° F. Delirium commenced on the evening of the fourth day; violent headache, with great restlessness, during the fifth. Throughout the sixth day he continued raving, constantly screaming out and tossing to and fro. During the following day to these symptoms were added considerable prostration, convulsive twitchings, and convergent strabismus. Early on the morning of the eighth day of his illness he died. The discharge from the ear had ceased two days previously.

As regards treatment, upon the supervention of intracranial inflammation repeated leeching was employed over both mastoid processes, while antimony was administered internally, and subsequently mercury both by the mouth and by inunction.

The autopsy revealed arachnitis both of the surface and base of the brain, with considerable effusion of lymph in the subarachnoid spaces. An extensive comminuted fracture traversed the petrous portion of the left temporal bone. Passing upwards from the jugular fossa, across the internal auditory meatus, it freely opened the subarachnoid cavity. From the meatus it radiated both outwards and inwards. Externally, passing through the labyrinth, tympanum, membrana tympani, and external meatus, it terminated above in the pars squamosa. Internally it traversed the body of the sphenoid, crossing successively the left middle lacerated foramen, sella turcica, and right middle lacerated foramen, on the far side of

which it ended. A connecting line of fracture on the left side united the middle lacerated foramen with the jugular fossa by running down along the inner border of the pars petrosa in the track of the inferior petrosal sinus. These fractures passed through the whole thickness of the bones. In the preserved specimen, which I exhibit, the largest of the comminuted portions, which is formed by the apex and adjacent portion of the pars petrosa, is distinctly movable. The part of the fracture which ran through the different bony chambers of the left ear, connecting the internal with the external auditory meatus, had permitted the free exit of the subarachnoid fluid immediately after the accident.

I availed myself of the opportunity, which was afforded me at that time, to ascertain the chemico-physical characters of the cerebro-spinal fluid, and specially as regarded sugar. For this purpose I submitted the fluid, which I collected fifteen hours after the accident, and previous to the supervention of intracranial inflammation, to Prof. Emerson Reynolds. He informed me that it contained sugar, and a slight but distinct trace of urea. Prof. Cameron kindly supplemented this statement by a quantitative analysis with the following results:—" Sp. gr. 1·007; reaction decidedly alkaline; nearly colourless; froths slightly on agitation; 100 parts contain—

Water	. . .	98·630
Albuminoids	. .	·488
(Including urea ·061.)		
Mineral substances	. .	·698
(Including sodium chloride ·630)		
Sugar	. . .	·166
Substances soluble in ether	.	·018 "

The presence of sugar in the cerebro-spinal fluid, as a normal condition, was propounded by M. Claude Bernard[a] in his Physiological Lectures. He stated that the sugar came from the liver; that anything which increased or diminished the glycogenic secretion of this organ, proportionally increased or diminished the amount of sugar in the cerebro-spinal fluid; and that, while section of the pneumogastrics arrested, injuries of the fourth ventricle favoured its formation. Alluding to the presence of sugar in the aural discharge in cases of fractured base, M. Claude Bernard held that, while the assumption of a traumatic diabetes was a possible mode of explanation of its occurrence in such cases, yet it should be

[a] *Cf.* Leçons de Physiologie Experimentale. 1855. Tome I., p. 315.

remembered that sugar was a normal component of the cerebro-spinal fluid. If this experimental observation were true, we ought, by testing for sugar, to have a ready mode of determining the cerebro-spinal nature of the watery discharges in fractures of the skull. Such is clearly Mr. Erichsen's view when he remarks that "an additional point of analogy between this discharge in fractures of the skull and the cerebro-spinal fluid is to be found in the fact pointed out by C. Bernard, that they both contain a small quantity of sugar."[a] Other surgical authors, however, among whom I may specially notice Mr. Prescott Hewitt, and Mr. Le Gros Clarke, make no allusion to the presence of sugar in the watery discharge.

These circumstances induced me to have the aural discharge carefully analysed for sugar in the case I have mentioned. The two eminent chemists to whom I submitted the fluid each determined its presence, and I on several occasions found it give the characteristic reaction with Fehling's cupropotassic solution. It may be said that the reduction of oxide of copper by the fluid was no necessary proof of its containing sugar. This objection has been raised; for it has been found by some observers[b] that the cerebro-spinal fluid, though giving the usual reaction with Trommer's test, has no power of deflecting the plane of polarisation in the sacchari-meter, and does not with yeast yield the usual products of fermentation—carbonic acid and alcohol. Prof. Hoppe-Seyler,[c] however, who has investigated this point with care, relies upon the positive results he has obtained by Trommer's, Moore's, and Böttger's tests as conclusive that the substance in question is a form of sugar, though it did not possess the property of rotatory polarisation, and did not undergo alcoholic fermentation. The fermentation test so far yielded him a positive result that the substance disappeared, though without evolving carbonic acid and alcohol. In this con-nexion I may remark that Prof. Nacquet[d] has indicated the existence of two glucoses, sorbin and eucalyn, which differ from the other glucoses in these very particulars—namely, that with yeast they do not ferment at all, or only under special conditions, and that they do not possess the property of rotatory polarisation. The sugar in the cerebro-spinal fluid would, in such cases, appear

[a] *Cf.* Surgery. Vol. I.
[b] *Cf.* W. Kuhne. Physiologische Chemie. 1868. P. 267.
[c] *Cf.* Virchow's Archiv. 1859. Bd. 16, p. 397.
[d] *Cf.* Chemistry. P. 466.

to be allied to these exceptional forms of glucose.[a] In my case there was evidence of the presence of a form of sugar allied to levulose, or fruit-sugar; for, besides affording positive results with the ordinary chemical tests, such as Trommer's, the fluid when examined in the saccharimeter, after removal of the albuminoids, distinctly rotated the plane of polarisation to the left.

The relatively large amount of a lævogyrate sugar observed in the cerebro-spinal fluid in the case I have detailed, I am inclined to consider as more than physiological, and to regard it with the vomiting, obstinate constipation, and urinary retention, as indicating the severe injury of the brain-substance which resulted in fatal intracranial inflammation.

I further submitted for analysis some of the urine and blood removed at the same time as the aural discharge; but in neither of these fluids was any appreciable trace of sugar detected. This is of some interest, as I am not aware that such supplementary examination of the urine and blood has been instituted in similar cases. M. Bernard specially remarks upon the want of such evidence. The question now arises—is the absence of sugar in the urine and blood necessarily antagonistic to the theory whereby the presence of sugar in the cerebro-spinal fluid would be ascribed to the brain injury? If the sugar in the cerebro-spinal fluid was owing to the injury of the brain, should sugar also have been present in appreciable quantity in the urine and blood? I do not think that this conclusion necessarily follows.

It will be conjectured from the preceding remarks that I do not hold that the cerebro-spinal fluid in its normal state always contains sugar. Mr. Lucas[b] has recorded a nearly unique[c] instance of a fluctuating tumour attendant upon fracture of the temporal bone. By aspiration it was found to be composed of cerebro-spinal fluid. The careful analysis which is appended leaves no room for doubt as to its nature. It is expressly added that it was free from sugar. Dr. Bennett also has communicated to this Club, some years ago, the result of his analysis of cerebro-spinal fluid, withdrawn from a congenital meningocele. He stated no sugar was found. Lastly, the researches of Prof.

[a] I have received a communication from Prof. Hoppe-Seyler to the effect that he has, in further analyses, found this substance in the cerebro-spinal fluid to yield all the reactions of grape-sugar, and that he has only ascertained its presence in this fluid when cerebral irritation existed.

[b] St. Thomas's Hos. Reports. 1875.

[c] Cf. Lancet. July 17, 1869. P. 79.

Hoppe-Seyler are singularly conclusive. The fluid withdrawn in two cases of spina bifida, on different occasions, at one time yielded, at another time failed to yield sugar. The fluid withdrawn in cases of chronic hydrocephalus manifested the same instability regarding sugar. These facts prove that sugar cannot be regarded as a constant constituent of the cerebro-spinal fluid, and that its presence or absence cannot be predicated; since in the same individual at one time it may be present, at another absent. It follows that the diagnosis of the cerebro-spinal nature of the watery discharges in fractures of the skull must be founded on chemico-physical characters other than that of the presence of sugar. The very low specific gravity, almost complete absence of albumen, and comparatively large proportion of sodium chloride, are the constant characteristics of the cerebro-spinal fluid.

ABSORPTION BY THE MUCOUS MEMBRANE OF THE VAGINA.

E. HAMBURGER, of Prague, made the following experiments with a view of elucidating this subject. Plugs of cotton impregnated with various solutions were introduced into the vagina and allowed to remain for twenty-four hours; the urine was in the meanwhile drawn off by means of a catheter, and tested for the presence of the substance. The solutions introduced were potassium iodide, ferro- and ferrid-cyanide of potassium, salicylic acid, bromide of potassium, chloride of lithium, and several other salts of iron. All these substances could be detected in the urine in the course of two or three hours and were still perceptible even after the lapse of twenty-four hours. The ferrid-cyanide of potassium appeared here first, as takes place after its absorption by the stomach in the form of a ferro-cyanide. It is not to be supposed that there is a reduction of this salt, but it is decomposed by the presence of other organic compounds. Hamburger believes it to be due to the uric acid present in the urine. He was unable to discover the iron compounds in the urine, and he had previously been unable in experiments undertaken with Schroff to discover it in the urine when administered by the stomach. On the other hand, he was always able to demonstrate its presence in the ashes of urine, and quantitative analysis showed that it was present in larger quantities than in the urine of healthy individuals. It is probable, he thinks, that the iron is not present in the form of a simple salt, since it would certainly be discoverable by the highly delicate ammonium sulphide reaction. This salt permits the presence of iron to be discovered when only 0·00018 of a gramme of the metal is present in 100 cub. cms.—*Prager Vierteljahrschrift*, B. CXXX., p. 245, and *Practitioner*, October, 1876.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

The Natural History and Relation of Pneumonia to Clinical Study.
By OCTAVIUS STURGES, M.D., F.R.C.P. London: Smith,
Elder & Co. 1876.

WE must apologise to our readers and to Dr. Sturges for not
having sooner noticed this valuable and interesting work, now a
year old.

Those of our readers who have paid any attention to the study
of pneumonia are familiar with the name of Dr. Sturges as a care-
ful and energetic writer upon this disease. Dr. Sturges, in the
present work, brings before us the result of ten years' laborious
study of inflammation of the lungs, combined with a careful criti-
cism of the views at present held with regard to pneumonia and
allied forms of disease. Dr. Sturges traces the history of pneu-
monia through all the ages of medical literature, and points out
that, until comparatively recent times, it was confounded with,
and, so far as diagnosis is concerned, undistinguished from pleu-
ritis. Pneumonia has manifestly prevailed as a complication in
many of the epidemics of the Middle Ages, and probably even
at much earlier dates. Thus the plague at Athens, 430 years
before Christ, seems to have been a pneumonia, combined with
enteric fever. In the eighteenth century, at Rome, there pre-
vailed epidemics of the sthenic and asthenic type:—

"In the first, which was of an 'inflammatory character,' bleeding was
beneficial. In the second (in 1713) gangrene was apt to supervene both
in the lungs and other organs, and now the operation of bleeding, useful
before, was believed to be fatal."

A better illustration of the mistake of saying, as many do, that
bleeding and the use of wine are mere fashions, each of which has
had its turn, could scarcely be found than the foregoing quotation.
We cannot follow Dr. Sturges through the whole of his chapter on
"Pneumonia in History," admirable and interesting though it be.

Dr. Sturges does not adopt the modern classification of the varieties of pneumonia into croupous, catarrhal, and interstitial. With a view of classifying the various forms of pulmonary consolidation known as pneumonia, he examined the books of St. George's Hospital for twenty years, and came to the conclusion that all cases of lung consolidation must fall under one of four classes:—The *first* comprises " patients who died of tedious and exhausting diseases of whatever kind;" the *second* consists of " the subjects of a specific fever, or of some definite affection of a secreting organ, and conspicuously of uræmic poisoning;" the *third* class—hepatisation—seems " due almost entirely to mechanical causes, such, for example, as would arise from defective power of the heart," &c.   " *Fourth*—Hepatisation occurs in connexion with that disease which the prevailing nomenclature describes as ' acute sthenic exudative pneumonia.' "

" Of the four classes the last is the only one which can lay claim to distinctive features, or be classed and described among diseases as having a history of its own."   Hepatisation in the other classes is an accident.

From the above quotations it is evident the author casts aside as *pneumonias* those forms known as interstitial and catarrhal.   The author having thus cleared the ground, proceeds accurately to detail the natural history of pneumonia, and to draw a picture of what is previously referred to as " acute sthenic exudative pneumonia."   When discussing the various forms of pulmonary consolidation which may possibly be included under the head of pneumonia, Dr. Sturges concludes as follows:—

" In determining the general canon which is to judge them, we must be mindful that the same sharp line of difference, which by our definition would seem to separate pneumonia from its counterfeits, will be lost now and then in actual practice.   There is an ill-defined middle territory which, if it cannot belong rightly to both, must yet remain the subject of a disputed ownership.   While, therefore, here or elsewhere room must be given for anomalous and exceptional cases—since a perfectly rigid classification in practical medicine is sure of failure—it is yet possible, as I believe, to lay down with some precision that which constitutes necessarily, so far as experience serves, the objective phenomena of pneumonia."

In considering the causes of pneumonia, Dr. Sturges points out that, although pneumonia is to a great extent dependent on

meteorological influence, yet it is but little dependent upon cold when compared with bronchitis, and says:—" As regards the influence of temperature, it would seem that cold does not necessarily affect the pneumonia rate, but always and markedly the bronchitis rate." High winds from the north and north-east, combined with a low, yet not necessarily a very low, temperature, favour the development of pneumonia. Dr. Sturges' observations are quite in accordance with those of Drs. Grimshaw and J. W. Moore, published in this Journal in March, 1875.

Having indicated the main lines of Dr. Sturges' work, we must conclude by thanking him for the great service he has done in writing in a definite way concerning *pneumonia*. The term has become, in clinical teaching, a sort of bugbear to students, who in many instances have come to the conclusion that pneumonia, catarrh, and phthisis, are so intimately connected that they cannot be separated either clinically or pathologically. We must refer such students to Dr. Sturges' work, after the perusal of which they will have much more definite ideas, not upon pneumonia alone, but also upon most of the other forms of acute and chronic pulmonary diseases met with in practice. It is much to be regretted that "pneumonia" should ever have been used as a generic term for lung consolidation, and have thus produced endless confusion in clinical medicine, and even in pathology.

T. W. G.

Elements of Physics, or Natural Philosophy. By NEIL ARNOTT, M.D., LL.D., F.R.S., &c. Seventh edition. Edited by ALEXANDER BAIN, LL.D.; Professor of Logic in the University of Aberdeen; and A. SWAINE TAYLOR, M.D.; F.R.S., &c. London: Longmans. 1876.

FIFTY years ago Dr. Arnott published the first edition of his " Elements of Physics." Few scientific works gained so wide and so well-deserved a popularity, and many editions rapidly passed through the press. Among all classes Arnott's Physics was the means of diffusing a sound elementary knowledge of natural philosophy in years gone by. Not long before his death, conscious of the rapid strides science had made since the first edition of his work, Dr. Arnott attempted to bring his treatise on Natural Philosophy into harmony with modern ideas. The result was, unfortunately, a sad and conspicuous failure. New cloth was tacked on to old

garments, and the rent was made worse. The writer of the present notice had occasion at that time to write an extended review of the two volumes, in which form Arnott's Elements of Physics then appeared; and the result of a careful examination of the work led to its detailed condemnation. Dr. Arnott had overtaxed his powers, and the revision should have been placed in younger hands.

The eminent editors of this edition, being Dr. Arnott's literary executors, " were charged by him to adapt the work to the present state of knowledge, while retaining in his own words all that was permanent in the doctrines and exposition of the subject." It may seem strange that a Professor of Logic and a Professor of Medical Jurisprudence should undertake the editing of a work on Natural Philosophy; but Professors Bain and Taylor have found a very able coadjutor in Mr. J. Cooke, M.A., formerly Assistant Professor of Natural Philosophy in the University of Aberdeen; and the result is an edition of which we can speak with the most hearty and unmingled praise. All the peculiar excellencies of Dr. Arnott's style and apt illustration have been preserved, but the scientific part of the work is almost entirely new, so that the most exacting critic will find it hard to detect a single departure from the strictest scientific accuracy. We have here, indeed, a book that has long been wanted—a thoroughly trustworthy, popular, treatise on Natural Philosophy. A treatise at once interesting and accurate, clear without being verbose, and popular without being shallow. The most recent developments of science have been introduced, but not to the exclusion of the solid basis that forms the foundation of natural knowledge. Perhaps the most striking feature in the volume is the fecundity of illustrations of the principles that are explained; this is traceable to Dr. Arnott's ever-watchful and keen observation of common things. Science is thus brought to bear upon the most familiar objects, and vivid ideas and a lively interest are thus aroused in the study of natural phenomena.

It will be impossible to give any adequate conception of the excellence of this work in a short notice like the present, but the following paragraphs, which form the headings to the section on energy, will show our readers the style in which the book is written, and at the same time indicate the precision of thought that characterises the pages of this new edition:—

" A man is said to have great bodily *energy* when he is capable of overcoming many obstacles, or of getting through a great amount of labour or *work*. His work is the measure of his energy, and the translation of

it into a visible form. . . . In mechanics, then, energy is the power of effecting work, or work is the overcoming resistance of any sort. . . . For the precise estimate of the amount of work-power in any moving mass we must agree on some definable amount of resistance offered to some definite quantity of matter or mass, and we might select for this purpose any constant and reliable resistance. . . . The resistance which the earth offers to the lifting of a given mass is invariable at the same spot on the globe, and is withal so convenient that it is greatly preferable to any other standard. The same amount of work-power was needed to raise the huge pile of the pyramids in olden times, by whatever means it may have been supplied, as would be required at the present day. . . . The unit of work is accordingly defined as the energy required to raise one pound of matter of any sort vertically through the space of one foot at London, and this is known as the foot-pound or unit of work in this country. . . . Work, as we have defined it, must, therefore, depend on two things for its measurement—(1) on the *weight* raised, and (2) on the *height* to which is is raised. Hence the simple rule—the whole weight raised in pounds multiplied by the whole vertical height in feet gives the amount of work done against the resistance of gravity in foot-pounds."

There is just one point we notice here. Why have the editors, after the clear definition they have given of force and energy, used the expression in the summary, on page 42, "the conservation of energy *or force ?*" The latter term is obviously incorrect; possibly their justification is to be found in the words that precede—namely, this great law "is usually referred to under" the expression we have quoted. We do not remember having ever met with so useful and comprehensive a table of the velocity of various bodies as that given on pages 43 and 44.

The outline of the work is as follows:—

There are altogether six parts in one volume, embracing nearly 900 pages. The first part deals with the constitution of the material universe—that is, the properties of matter and the properties of motion. The second part embraces elementary mechanics, the centre of gravity, and simple machines. The third part, the phenomena of pressure and motion in fluids, which includes a wide range of subjects—namely, *hydrostatics,* the laws of liquid pressure; *hydraulics,* the laws of liquid motion; *pneumatics,* gaseous pressure and motion; and *acoustics,* the phenomena of sound and hearing.

The fourth part is headed *imponderables!* This, surely, is an oversight on the part of the editors; for, when we turn to the page

referred to, we find this term set aside, and a clear exposition of the dynamic theory given in its stead. This heading, however, was in the earlier editions, and has, no doubt, been overlooked. It is misleading, and should be corrected. In this part we find the first section devoted to heat, the second to light, the third to electricity, and the fourth to magnetism. We are glad to find spectrum analysis, diffraction, and the polarisation of light have now found a place. These were omitted in the previous editions as beyond the scope of the treatise, which, nevertheless, was in its day presented as a text-book for students for university examinations!

Electricity is the weakest part of the volume—only 36 pages are devoted to the whole range of static and dynamic electricity. What is written is good so far as it goes, but we trust the editors will make a very needful enlargement here, for if 39 pages can be spared to magnetism and 176 pages to heat, certainly 150 pages should be given to electricity instead of 36.

The fifth part is devoted to astronomy, which, with our present notions of the range of physics, seems a little out of place in a treatise on natural philosophy; nevertheless this part is useful in giving a popular and broad outline of the elements of astronomical science.

The sixth and last part is occupied with animal mechanics and animal hydraulics and pneumatics, the former including the mechanism of our framework, and the latter the circulation of the blood and air, regarded chiefly in their physical aspects.

We have thus sketched, in brief outline, the contents of the volume before us; and, in conclusion, repeat that we can conscientiously commend this book to those of our readers who wish for a sound and readable elementary work on physics, viewed in its broadest aspects. Personally, the present writer is especially glad the opportunity has been afforded him of giving a hearty welcome to this new and excellent edition of Arnott's Elements of Physics.

W. F. B.

Biennial Retrospect of Medicine, Surgery, and their allied Sciences, for 1873–74. New Sydenham Society. London, 1875. Pp. 557.

THIS volume covers the same ground as its predecessors, and embraces, as usual, eight separate reports. The Report on Practical Medicine, by Dr. A. B. Shepherd, is carefully and elaborately

executed, the bibliographical lists being very extensive.   Some of
the other reports are weak and incomplete, and betray signs of a
random use of the scissors and gum-bottle on English journals,
while extracts from foreign sources are conspicuously rare.   What
a contrast to the magnificent summary of work, domestic and
foreign, furnished year by year in Virchow and Hirsch's *Jahres-
bericht*, by the most eminent men in their own departments.
Would not the funds expended by the New Sydenham Society in
publishing a poor and incomplete Biennial Retrospect be much
better devoted to re-issuing, in an English dress, the invaluable
German summary above mentioned ?

Laurella, and other Poems.   By JOHN TODHUNTER.   London:
        Henry S. King and Co.   1876.   8vo., pp. 275.

WERE we asked to name a study which would be at once a solace
and a recreation to the busy physician—harassed by frequent calls
upon his time and skill, wearied by daily contact with the sad
realities of life, and yearning for intellectual rest and refreshment—
we would unhesitatingly assign the palm to poetry.   By such a
study one is brought into a happier and healthier frame of mind,
and the pleasure experienced is infinitely enhanced should the poet,
whose works are read, be himself a physician.

   These reflections were suggested as, in the long winter evenings
which are passing away, we eagerly read and re-read the graceful
poems which Dr. Todhunter has lately published.   The gifted
author needs no introduction to Irish readers—certainly not to those
who are members of the medical profession.   Well known as a
sound, practical, and skilful physician, he has been for years
equally well known as a writer and poet.   Indeed, in the volume
before us, many poems are reproduced from the pages of *Kottabos*
and other periodicals and magazines.   But, in addition to these,
several more ambitious works now see the light for the first time.

   Foremost among the new poems we find a series of " Tales "—
" Laurella," " The Daughter of Hippocrates," and " The Lost
Violin-theme."

   The subject of " Laurella" is taken from Paul Heyse's prose
tale, *La Rabbiata*.   It is a Byronic poem in a certain sense—that is
to say, in the wonderful rhyming-power displayed by its author, and
in the general treatment of the subject.   Many of the " reflective"

passages are equal in power to " Don Juan," but all is chaste
and pure, in direct contrast to that " truant offspring of Lord
Byron's muse," that " Odyssey of immorality," as the critics had it.

As an illustration of the " Don Juan " ring of the poem, we may
quote four lines which are singularly *àprupos* in these days of
" women's rights " and women-doctors :—

> " To see (O tempora !) well-tamed womankind
> Turn restive, tear the yoke from their soft necks,
> Kick up their heels, and, like Laurella here,
> Bolt from that paddock blandly termed ' *their sphere !* ' "

The second " Tale," " The Daughter of Hippocrates," is based
on the legend as told by Leigh Hunt in *The Indicator.* In the
third " Tale," " The Lost Violin-theme," we, for the first time,
meet with the expression of that passionate and intelligent love of
music which runs like a golden thread throughout the volume.
Indeed, many of the subsequent pieces are suggested by compo-
sitions of the masters, Beethoven and Mendelssohn. For example,
among the " Miscellaneous Poems " we find " In a Gondola,"
suggested by Mendelssohn's Andante in G minor, Book I., Lied. 6,
of the " Lieder ohne Worte," and among the " Primitiæ " a
" Moonlight Sonata " (not, however, Beethoven's) is introduced
with all its movements. In the " Sonnets," too, there are addresses
to Mendelssohn and Rossini, and the poet tells how Beethoven
speaks for him.

Another striking characteristic is the love for nature which
bursts into song in almost every page :—

> " Spring, Spring, sweetest Spring,
> How shall I give thee welcoming ?
> When thy blue eye peeps from the sky,
> Larks must sing, and so must I ! "

And again :—

> " To-day the Spring is in the air
> And in the blood : sweet sun-gleams come and go
> Upon the hills ; in lanes the wild-flowers blow,
> And tender leaves are bursting everywhere.
> About the hedge the small birds peer and dart ;
> Each bush is full of amorous flutterings
> And little rapturous cries. The thrush apart
> Sits throned, and loud his rich contralto rings—
> Music is on the wind, and in my heart
> Infinite love for all created things."

God grant us the poet's heart !|

But this book contains intellectual food of another kind also, and

to some readers the group of poems included under the heading, " The Mystic," will, possibly, have greater charms than those already quoted. " There shall come false Christs " is a rebuke to the narrow-minded, intolerant, and false notions of the age, on subjects connected with the present and the future destiny of man. Truly does the poet say:—

> " When things that be are as things that seem,
> Then all the world will have dreamed this dream!"

Lastly, the reader will derive much pleasure from the perusal of a " Daigntie-Conseated Sonnet," the " Masque of Psyche," and the " Ode to Dyspepsia," especially should he be unhappily haunted by that

> "Accursèd Hag! Hell-conceived, fury-born,
> Twin-sister of the fiend Despair."

In heartily congratulating Dr. Todhunter on the beauty of his poems, and in as heartily recommending this volume, we cannot refrain from quoting, in its entirety, the sonnet, " Nessun maggior' Dolore ":—

> "No greater grief! Is it then always grief
> Remembering happier times in times of sorrow ?
> Does one day of delight ne'er bring relief
> To the sick soul on a despairful morrow ?
> Past joys are a possession. Oft we borrow
> Strength for our present pain from out the brief
> Bright moments garnered long in memory's sheaf—
> August's rich grains make glad December's furrow.
> Have once mine eyes beheld in vision blest
> Beauty's dread form, or Love's death-conquering face,
> My heart leaped up transfigured, as she sung,
> Who raised to life my life, whose gentle breast
> From the world's rush was my one resting-place,—
> Blind, deaf, and old—I see, hear, still am young."
>
> <div align="right">J. W. M.</div>

WORKS ON MEDICAL ELECTRICITY.

A Practical Treatise on the Medical and Surgical Uses of Electricity. By G. M. BEARD, M.D., and A. D. ROCKWELL, M.D. Second Edition. London: H. K. Lewis. New York: Wood & Co. 1875. Pp. 794.

THERE is a vast difference in the amount of material in the two works before us. Nearly 800 pages of closely printed royal octavo demand some determination on the part of the reader to face them. In 1871 Drs. Beard and Rockwell brought out the first edition of

their work, which was soon exhausted, and from that time forward " much force (!) has been expended on the thorough revision of the work in all its departments." Unfortunately it has not only been revised, but also considerably enlarged, and the result is a bulky volume upon which much labour has evidently been bestowed. The book contains a great deal of practical information, and appears to aim at an encyclopædic survey of its department. Yet although it may undoubtedly be useful to specialists, we cannot cordially recommend it, disfigured as it is by so many blemishes. The success of the first edition, together with the flattering testimony of a German translation by Dr. Väter, seems to have turned the heads of the authors, and to have tempted them to let loose an unbridled pomposity of diction and an immoderate conceit, which not only inevitably excite a feeling of ridicule, but also invite criticism of the wonderful results which they confidently claim to have obtained by their methods.

Did space permit we could quote numerous specimens of vulgarity of expression, and of that grandiloquence which an American reviewer of the book characteristically stigmatises as the "spread-eagle style," quite unworthy of a scientific treatise. We may refer the curious to pp. 48, 118, 245, 275, 278, and 391.

The section on electro-physics is very poor, *pace* the flourish with which it opens, and it is difficult to understand how M. Leclanché's name is nine times misspelt *Lechanché* within three pages. The most useful chapter in this section is that on Ohm's Law, which is now fully discussed in its practical applications to electro-therapeutics, whereas in the first edition it was dismissed with a few lines. The authors seem to borrow much of their physiology from that most perplexing of books, " Morgan's Electro-Physiology and Therapeutics, 1868," and it is difficult to sift out the few grains of certain knowledge from the heap of chaff in which they are enveloped. At p. 155, in speaking of the action of the galvanic current on the motor nerves of healthy man, the extraordinary statement is made, without any qualification, that "at the opening of the current there is no contraction." But a still more astonishing error, coming too from specialists, meets us in the section on Facial Paralysis. Speaking of the diagnosis of Bell's paralysis, we read—" The fact that in facial paralysis of central origin the electro-muscular contractility is unimpaired, while if the nerve itself is the seat of the injury the *muscles refuse to respond to either current* (!), materially aids us in diagnosis," p. 519.

The chapter treating of the authors' special method of "general Faradisation," for which they modestly claim to have done what Duchenne did for localised Faradisation, is visionary and specialistic in the worst sense. The therapeutical part of the work, occupying nearly half of the entire space, is weak and inconclusive. It is burthened with the reports of 250 cases of singularly little clinical value, and in many instances badly reported, while a plausible show is made of recording unfavourable as well as favourable results. In conclusion, while there can be no doubt that the authors have had a large experience, and that they evince a considerable knowledge of practical details, we are bound to say that they have not succeeded in supplying the desideratum of a compendious, accurate, and scientific treatise on medical electricity.

A Text-Book of Electricity in Medicine and Surgery. By G. V. POORE, M.D. London: Smith, Elder, & Co. 1876. Pp. 291.

DR. Poore's book is much less pretentious than the preceding one, and is, we think, successful in its aim of furnishing, within a moderate compass, a practical guide for students and practitioners. Quotations from the writings of others are freely interspersed, and the style bears some marks of indigestion and hasty compilation; but the book also shows evidence of real knowledge, and contains the record of some original observations of practical value.

Dr. Poore has studied with considerable care the subject of chronic fatigue, particularly the condition commonly known as " writer's cramp;" and the cases related, illustrated by facsimiles of the patients' handwriting, give much encouragement to undertake the treatment of this singular and hitherto intractable malady. There is good sense, too, in a method of using galvanism which is specially advocated by the author—*i.e.*, the *combination of a continuous galvanic current with voluntary muscular exercise.* In more than one case we have felt grateful to Dr. Poore for this hint of utilising the co-operation of the will with the application of the electrical stimulus to the muscles. As to the pretended electric stimulation of the cervical sympathetic, we imagine that most unbiassed observers will coincide in the opinion of the author, who has " galvanised the sympathetic many times, but beyond the occasional dilatation of the pupil (which is by no means an inevitable occurrence), he has never seen any indication of the nerve responding to the stimulus."

Of the different forms of galvanic cell we certainly hold with Dr. Poore that, up to the present, the Leclanché element is far the best for medical purposes. Dr. Poore's experience of it dates from 1874, but we have had it in constant use since 1870, and have no hesitation in expressing our unqualified admiration for its cleanliness, convenience, and extraordinary durability.

We have much pleasure in recommending this work to our readers as a trustworthy manual, and have little doubt that a second edition will shortly be called for.

Clinical Class Note-Book.—Surgery. By Professor MACLEOD. Glasgow: James Hadden, Bookseller, 129, Sauchiehall-street. Pp. 15.

THIS little work is one which we recommend very strongly to the notice of clinical teachers of surgery. It is, in fact, a note-book arranged by Professor G. H. B. Macleod, of Glasgow, for his own clinical class. It is intended, as he says, to aid the student in the ward, by suggesting to him the more important points for observation in surgical cases.

It commences by giving, in the first three or four pages, some short instructions as to the general subject of case-taking. The special feature of the work is, however, the portion from this to the end, in which it simply gives, in as few words as possible, notes which are intended to remind the case-taker of the points which should be noted in each variety of surgical ailment—points, some of which may at times escape the most accurate clinical clerk.

To illustrate the method of using Professor Macleod's clinical class note-book—suppose a case of tumour of the breast is to be taken. The student, having first noted general particulars—name, age, sex, employment, date of admission, &c., turns to page 11, where he finds, under the heading " Mamma, tumours of," the following :—

" NOTE.—The more common are abscess (acute and chronic), cysts, cancer, adenoma, &c. The less usual are hydatids, fibrous, cartilaginous, and osseous growths. Observe age, appearance, and general health. Married or single. Number of children, and age of youngest. Any abortion or miscarriage. Has breast been used in nursing. Catamenia present or ceased, and if ceased, when. First appearance of growth, and assigned cause. Rate of progess. Pain or not. State of skin as regards colour and fixture to surface of growth. Condition

of the veins and glands. Mobility or fixture. Shape. Nipple normal
or retracted. Effects of traction on the nipple as regards the tumour.
Any discharge from nipple. Any ulcer or sinus, and if former, its
character. Fluctuation, weight, hardness, or irregularity of the growth.
Any œdema. Well or ill defined. Possibly puncture may be employed.
Is other breast sound?"

These notes on each topic are alphabetically arranged, and serve,
in fact, as short memoranda on all the various surgical affections to
remind the student of the points to be observed.

Each clinical class note-book is furnished with a case of boards
and paper for case-taking, and on the inside of the boards outline
figures of the human body are given, which can be traced on paper
so as to mark out the situation, relative size, &c., of tumours, or
region of pain, dulness, &c.

The note-book can be obtained of Mr. James Hadden, book-
seller, 129, Sauchiehall-street, Glasgow. Its moderate cost—two
shillings in boards and with paper, or one shilling without these
additions—places it within the reach of every student. We can
heartily recommend it to students, and also to such hospital
surgeons as are anxious to have good records of their cases kept by
their clinical clerks.

Kirkes' Hand-book of Physiology.   By W. MORRANT BAKER,
   F.R.C.S. Ninth edition. London: John Murray. 1876. 8vo.
   Pp. 859.

WITH the exception of the name this volume retains little of the
original work of Dr. Kirkes, the lines of which are, however, closely
followed by Mr. Morrant Baker. Of the present edition we are
able to speak in unqualified terms of commendation. All that is
certain in modern physiology is accurately described, while debat-
able ground is trodden with prudent and cautious reserve. In
illustration of this remark we may refer to the account of "Dia-
pedesis of Blood-corpuscles" (p. 197), on the one hand, and to this
paragraph respecting localisation of brain-function on the other
hand:—

"From the apparently greater frequency of interference with the
faculty of speech in disease of the *left* than of the *right* half of the cere-
brum, it has been thought that the nerve-centre for *language*, including
in this term all articulate expression of ideas, is situate in the *left* cere-
bral hemisphere. A large number of cases are on record in which

aphasia, or the loss of power of expressing ideas in words, has been associated with disease of the posterior part of the lower or third frontal convolution on the left side (Hughlings Jackson, Broca). This condition is usually associated with paralysis of the right side (right hemiplegia). The only conclusion, however, which can be drawn from this is, that the integrity of this particular convolution is essential to the faculty of speech; we cannot conclude that it is necessarily the *centre* for language. It may be only one link in the complete chain of nervous connexions necessary for the translation of an idea into articulate expression" (p. 549, *seq*).

Further criticism of a work which has reached a ninth edition is not required, and we would merely say that the book has been considerably enlarged, and is much more fully illustrated than former editions were. The plates on steel, however, are now omitted. A useful feature in the present volume is an appendix consisting of tables of various anatomical weights and measures, of the specific gravities of some tissues and fluids, of the composition of certain foods, and of the classification of the animal kingdom.

Landmarks — Medical and Surgical. By LUTHER HOLDEN, F.R.C.S. Pp. 74. London : J. & A. Churchill. 1876.

THE series of papers now collected in book-form first appeared in " Saint Bartholomew's Hospital Reports." We have already read them with great interest and advantage, and we can promise the same, we think, to most persons who may now consult this enlarged edition. As a student, we felt the great omission, on the part of clinical teachers, in directing attention to matters which, though elementary, are of the first importance in the observation of disease. Of all the pupils who can talk glibly about the pathology of pneumonia, bronchitis, or mitral disease, our own experience is that a very small proportion could carry out even a tolerably accurate examination of the thorax or abdomen. They could not point out the ordinary position of the valves of the heart with reference to the parietes of the chest, or the localities usually occupied by different organs. These are defects to be laid at the door of teachers rather than of students. Some of our transcendentalism might with advantage give place to what is simpler and more practical; and to those who wish to adopt this principle in teaching, we do not know of any book which will give them more reliable aid than this. It deserves a very large circulation among students as well as practitioners.

PART III.

HALF-YEARLY REPORTS.

REPORT ON OPHTHALMIC SURGERY.

By HENRY WILSON, Fellow and Councillor, Royal College of
Surgeons; Professor of Ophthalmic and Aural Surgery; Senior
Surgeon, St. Mark's Ophthalmic Hospital; University Examiner
in Ophthalmic Surgery, &c.

CONGENITAL MALFORMATIONS AND DISEASES.

Congenital abnormalities of the organs of vision are attributable
to arrest of development in the embryo or to pathological processes
in utero.   In the consideration of these conditions due to arrest of
development many puzzling questions crop up, some of the most
interesting and obscure, especially to the embryologist, arising in
the consideration of coloboma of the choroid, an anomaly usually
associated with coloboma of the iris.   The generally accepted
doctrine teaches that the original basis for the so-called choroidal
coloboma consists in fissure of the retina of the unclosed fœtal cleft
of the secondary ocular vesicle, and that the irregularities in the
choroid, iris, sclera, vitreous, etc., are consequent thereon.   After a
time the fissure is closed by cicatricial tissue, increased intra-ocular
pressure occurs, in consequence of which the yielding cicatricial
(connective) tissue is distended, and a staphyloma results, which
continues to increase after birth.   Professor Manz[*] communicates
a most interesting *anatomical examination of a coloboma of the iris
and choroid*, the most important result of which was the finding of
retinal structure in the coloboma.   At the margin of the coloboma
the retina undergoes a change, and within the space of the coloboma
loses altogether some of its layers—viz., the outer ones, pigment-
epithelium, bacillary, and outer granular layer and external limiting
membrane; while other layers are either very much thinned or
altogether deficient in some of their elements; the layer of optic

[*] Zehender's Klinische Monatsblätter für Augenheilkunde. 1876.

fibres is thickened, but so altered as to be represented by connective tissue only, the nerve-fibres bending in another direction and becoming replaced by connective tissue. In explanation of the existence of retinal tissue in the cicatricial structure, Professor Manz supposes—1st. Either that the retinal fissure had, though late, closed, but that the retina had not been normally developed at this place, or had become subsequently destroyed—or, 2nd. That the fissure remained, and became closed by heterogenous cicatricial tissue on which no retina was developed, but into which, partly by contraction, partly by expansion of the cicatrix, adjoining portions of retina had been drawn. The Professor is inclined to the latter theory.

CONGENITAL BUPHTHALMOS.

Dr. Fritz Raab[a] in his *Beiträge zur Pathologischen Anatomie des Auges*, communicates the macroscopic and microscopic details of an eye affected with congenital hydrophthalmos, a condition due in all probability to pathological processes in utero as distinguished from faulty or arrested development. The eye was removed from a boy, aged ten, on account of increasing and painful glaucoma. The globe had increased in size equally in all directions, and this increase in volume was accompanied by a thinning of the enveloping membranes at the corneo-sclerotic junction, and filling up of the space in that position, whereby the shape of the eye-ball became in general globular. Intimately connected with these alterations in shape and size were alterations of the topographical relations of the contents of the globe—viz., increase of the anterior and posterior chambers, advance of the iris, and recession of the lens. The author refers the original disease to the vascular ciliary body, and traces all the mischief to it, inasmuch as the inflamed ciliary body furnishes an exudation more or less rich in cells, which permeates the iris in its immediate vicinity and accumulates in the space of Fontana, giving rise to adhesion of the iris and cornea at the periphery, which increasing in extent causes the iris to advance and the pupil to dilate, the remainder of the iris being deprived of its nutriment atrophies. The sclerotic in those cases presents isolated patches of atrophy or of staphyloma in which the sclerotic tissue is sometimes altogether absent.

[a] Ibid.

ANOPHTHALMOS, WITH CONGENITAL CYSTS IN THE LOWER EYE-LIDS.

M. De Wecker[a] gives the particulars of a boy, aged six months, in whose orbits no eyes could be detected, but in whose lower lids were tumours containing a transparent fluid resembling in some respects aqueous humour. The author hopes at some future time to obtain further information respecting those remarkable cysts.

CONGENITAL ENCEPHALOCELE.

F. Raab[b] details a case of the above, which he met with in De Wecker's clinique in Paris, in the person of a man, aged twenty-three and a half, whose retarded development, broad skull, and prominent frontal protuberances denoted hydrocephalus. On the left side a tumour about the size of a hen-egg, covered with normal skin, occupies the inner angle, exhibiting in its lower half pulsation synchronous with the carotid. The eye-ball is pushed outwards and a little downwards. Pressure does not alter the size, and respiration does not affect it. Atrophy of the left optic disc caused great impairment of sight. When nine years old the tumour had been punctured, and a quantity of serous fluid evacuated, without, however, any bad results. The author quotes nine other cases as the only ones known to him in the literature on this subject.

DERMOID OF THE CONJUNCTIVA.

Dr. Reich[c] gives the particulars of a patch of dermoid tissue, situated in the lower lid of a soldier, surrounded by healthy conjunctiva, and which was in immediate continuity with the caruncula lachrymalis. Small yellowish acini, with light-coloured, delicate, and very yielding hairs interspersed, were observed in this island.

SUPERNUMERARY LACHRYMAL PUNCTA.

Dr. Fitzgerald,[d] of Dublin, publishes the details of two cases of the above which had come under his observation. In both instances there existed a separate and distinct canal leading from each puncture into the lachrymal sac.

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[a] Ibid. October, 1876.
[b] Ibid, and Wien. med. Wochenschrift. 1876.
[c] Ibid. P. 299. 1876.
[d] Royal London Ophthalmic Hospital Reports. May, 1876.

CONGENITAL MALPOSITION OF LENSES.

Mr. Streatfeild[a] figures and describes the eyes of a girl, aged nine, in whom the lens was in each side considerably misplaced or dislocated upwards. When the pupils were dilated this faulty position became very apparent, and on ophthalmoscopic examination the remarkable double image of the fundus (which was described by me in certain cases of luxation of the lens, in the August number of this Journal for 1865) became very manifest, the refraction being myopic when seen through the lens, and hypermetropic in that part of the pupil where the lens was absent. An iridectomy downwards and adaptation of suitable convex glasses rendered Mr. Streatfeild's patient's vision remarkably good and useful.

NEW OPERATIONS FOR GLAUCOMA.—SCLEROTOMY VERSUS IRIDECTOMY.

Under this heading Mr. Bader,[b] Ophthalmic Surgeon to Guy's Hospital, advocates the substitution of section of the sclerotic for iridectomy in cases of glaucoma, &c. The section is made by passing a Graefe's knife across the anterior chamber and cutting it out as near as possible to and immediately in front of the iris, and then withdrawing it without making a conjunctival section. The extent of sclerotic to be divided is equal to nearly a third of the circumference of the cornea. A large bridge of the conjunctiva is left, beneath which the aqueous humour escapes.

TREPHINING THE SCLEROTIC—A NEW OPERATION FOR GLAUCOMA.

It is universally agreed that the symptoms of the so-called glaucoma are due to increased intra-ocular pressure, and the object of all treatment should therefore be to lessen and reduce this increased tension. The old operation consisted in tapping the anterior chamber through a simple incision in the cornea, the modern in iridectomy; division of the ciliary muscle is also still practised by a few operators, but is generally abandoned, as wounds in the ciliary region are very dangerous. There are occasionally exceptional cases in which iridectomy, otherwise so remarkably beneficial, cannot be effected, and hence Dr. Argyll Robertson[c] has, in

[a] Ibid.
[b] Ophthalmic Hospital Reports. May, 1876.
[c] Ophthalmic Hospital Reports. May, 1876.

seeking for some other means of alleviating the pain and retaining vision, devised a method of drilling a circular aperture about $\frac{1}{13}$ of an inch in diameter through the sclerotic, through which aperture escape of some of the superabundant fluid in the chamber of vitreous humour is permitted. The instrument used is a modification of Mr. Bowman's cornea trephine. So far as his limited experience (four cases) goes, Dr. Robertson is in favour of this operation in certain cases, for he has found in it an effectual means of reducing increased intra-ocular tension, and he believes it will be effectual in preventing a return of increased tension.

DRAINING THE VITREOUS CHAMBER.

M. De Wecker* passes metal threads through the coats of the eye into the vitreous chamber and unites them outside, so that they act as a seton in a somewhat similar manner to what I have known the late Dr. Jacob do, many years ago, with silk ligatures in cases of staphyloma. I have tried this method, and, so far, with fairly satisfactory results.

THE INTRODUCTION OF THE METRICAL SYSTEM INTO OPHTHALMOLOGY.

In olden times opticians had an arbitrary system of numbering lenses, which has given way in modern times to numbering according to the focal distance of the lens or of the refracting power; the unit being represented by a lens which had a focal distance of one inch. Beside other inconveniences one of the principal ones of this system consisted in the fact of the inch being not a uniform measure corresponding to a known magnitude universally adopted; the English, Parisian, Austrian, and Prussian inches all differing materially in their quantities. To remedy these inconveniences the International Ophthalmological Congress appointed a Commission, and finally, in 1875, a new system was adopted, of which the following are the principles:—

a. The substitution of the metre for the foot as a basis for numbering glasses.

b. Numbering the glasses according to their refracting power, and not by their focal distance.

c. The choice of a unit sufficiently feeble, so that the numbers of the lenses generally in use may be expressed in whole numbers

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* Clinique Ophthalmologique du Dr. De Wecker à Paris. Par le Dr. Masselon. 1876.

and not in fractions, the interval being as far as possible the same between the different numbers.

The unit of the new system, the No. 1 of the new series of glasses, is a lens with a focal distance of 1 metre. It is called a Dioptric (D). Its refracting power is, therefore, expressed by the fraction $\frac{1}{1m}$. No. 2 is a lens which has two units or double the refracting power = ($\frac{2}{1m}$ = 2 D).

Professor Landolt, of Paris, contributes to the various ophthalmological periodicals[a] a lengthy paper explanatory of the new metrical system, and recapitulating certain known laws respecting refraction and accommodation. One of the greatest scientific advances of the present day is the correction of errors of refraction; in former years oculists knowing but little of these errors of refraction and of physics left the choice of spectacles altogether to the optician, who, no doubt, in many instances empirically suited the patient with spectacles, but who must often have failed to do so. Now-a-days the oculist investigates the faulty eye, measures its errors, and prescribes suitable optical correctors of the faults, analogous to the physician prescribing certain medicines to allay certain symptoms. Many persons who consider themselves blind of one eye are in reality not so, for often a properly adjusted. lens enables an eye, considered by its owner of no use, to read. While the optician's trade has been greatly increased by the oculist, his labours have been also lightened. That there should be a uniform standard of measurement then, all over the world, is a matter for congratulation. There will, no doubt, be some confusion during the transition period. For some time past I have been using a case of the metrical system lenses, but in prescribing lenses according to that system, I note also the equivalent in the old system.

DETERMINATION OF THE REFRACTION OF THE EYE BY MEANS OF THE OPHTHALMOSCOPE.[b]

From the foregoing it will be manifest that instrumental aid is an essential necessity to the oculist, and amongst the first and best instruments to enable the practitioner to determine the art and degree of the refractive anomaly he has to deal with is that of Dr. Loring, who has substituted for the clip in Liebreich's

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[a] Annales D'Oculistique. Zehender's Monatsblätter Ophthalm. Hosp. Reports, &c.
[b] By Edward G. Loring. New York. 1876.

scope detachable cylinders, which carry convex and concave glasses. Dr. Loring's *brochure* from advanced sheets on *The Ophthalmoscope* is a most valuable and important essay.

Dr. De Wecker, as well as Dr. Landolt, have, also, both devised refraction ophthalmoscopes, the latter having two Rekoss discs which contain metrical lenses.

THE PHYSIOLOGICAL ACTIONS OF COLCHICUM.

THE most remarkable phenomenon that this agent produces is the complete loss of sensibility from paralysis of both the peripheric and centric nerve-endings. The reflex excitability is consequently abolished. On the other hand, the motor nerves and the muscles retain their excitability till death takes place. In many animals the paralysis is preceded by a stage of excitation; that in frogs may rise in intensity till there is an outbreak of tetanic convulsions. The circulation of the blood is only slightly disturbed by the action of the poison. The heart continues to pulsate even after the paralysis of the central nervous system has commenced. The blood-pressure long remains unchanged, only sinking when the animal is moribund, and in like manner the paralysis of the inhibitory nerves of the heart occurs at a late period. The respirations become gradually less and less frequent, until they are entirely arrested, so that we must conclude that there is gradually increasing paralysis of the respiratory centre. In warm-blooded animals, and especially in cats poisoned with colchicum, the mucous membrane of the whole gastric and intestinal tract is swollen and strongly injected, whilst the intestine contains bloody mucus. In consequence of this, there are diarrhœa, vomiting, and colicky pains during life. The cause of the congestion has not as yet been ascertained. The fibres of the splanchnic and abdominal branches of the vagus were not paralysed. The kidneys were strongly hyperæmic, and their secretions diminished. The action of colchicum takes place very slowly, death only occurring after several hours, and it is remarkable that, as was observed some time ago by Schroff, the amount of the dose has scarcely any perceptible influence on the intensity or rapidity of the action of the poison. A few centigrammes, and in cats even a few milligrammes, are sufficient to cause death, which results from arrest of the respiration, whilst the heart continues to beat for some time. In this stage tonic or clonic convulsions occur both in cats and rabbits, which Rossbach attributes to asphyxia. Doses smaller than are sufficient to cause death have scarcely any action.—*Pflüger's Archiv*, B. XII., p. 308, and *Practitioner*, October, 1876.

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PART IV.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

SAMUEL GORDON, M.B., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, January 3, 1877.

DR. HAYDEN, Vice-President, in the Chair.

The Cold Bath in Typhus Abdominalis. By ALBERT A. GORE, M.D.;
Surgeon-Major in charge, Military Prison.

DURING a visit to Germany, last autumn, I had an opportunity of seeing the antipyretic treatment of typhoid fever in very general operation, and apparently with favourable results. The bath used in military hospitals was upon wheels, and so easily moved to the patient's bedside when required. At Frankfort, Dr. Dietriche, Oberstatsarzt, 1st Hessian Infantry, informed me that among his cases the mortality had declined considerably since the introduction of the bath, but that his patients had a very decided objection to being placed in it. Dr. Ullman, Chief of the Military Lazarette at Neurenberg, in Bavaria, had also found immersion "a very good mode of treatment." The same opinion was expressed at Cologne and Berlin. In the Military Hospital at Berlin the cases were treated in a ward with open windows, the patients in winter being protected from the direct effects of the cold air by screens. Ice-bags were applied to the head to allay cerebral excitement and maintain the functions of the brain in a normal condition. On the other hand, in Brussels, the windows of the fever wards were kept carefully closed. Dr. Ullman had found salicin as useful as quinine as an antipyretic.

The following short note of the case of a soldier, admitted into the Garrison Lazarette at Neurenberg, 17th August, suffering from an attack of typhus abdominalis (as the disease is called in Germany), will serve to illustrate the immediate effect of the bath :—

His temperature on admission was 105·4° F. The first immersion

reduced the temperature to 103°. Within two hours it had risen to 104°, and was again reduced by a ten minutes' immersion to 101°, and so on, during the progress of the case (as was seen by the chart, kindly given me by Dr. Ullman), until the temperature ceased to exceed 102·3° F., an event which occurred on the 24th, or a week subsequent to admission, during which time the patient had been placed in no less than 29 baths. He was convalescent on the 28th, although the temperature had not as yet quite fallen to the normal standard of 37·5° C. The cases of other patients in the ward had been much more prolonged, and their charts extended to a considerable length.

Dr. Gordon, the President of the Society, told his hearers, at the opening of the session in the Richmond Hospital, that the treatment of fever by means of cold affusions was practised by Perceval three-quarters of a century ago in the Hardwicke Hospital. He proved that a temperature of 105° in the mouth might be reduced to one of 98° by the direct cooling of the surface and the withdrawal of its body-heat, the excessive production of which led to the death of the greater number of those who succumbed to typhoid fever, death resulting from paralysis of the heart or paralysis of the brain in such cases without any special complication. In 1861, Brand of Stettin, gave the first stimulus to the systematic trial of the cold bath by the publication of his researches. He was followed by Bartels and Jürgensen, of Kiel, who pointed out that if this energetic withdrawal of heat was to be followed by any marked results, " it must be repeated as often as the temperature of the body rose above a certain point, and also that patients endured this repeated withdrawal of heat without experiencing any bad results."

Liebermeister,[a] writing upon the influence of the cold bath upon certain complications of the diesase, states :—

a. We have no evidence as to whether perforation of the bowel is more frequent or otherwise.

b. Suppurative parotitis has greatly diminished.

c. Affections of the respiratory apparatus occur rather more rarely, and run their course with less severity.

d. Bed-sores are less frequent as sequelæ.

e. Relapses, and mortality after relapses, are rather more frequent in proportion as the antipyretic treatment has been the more systematically employed.

f. Gangrene of the peripheral parts is more common.

Although hæmorrhage from the bowels constitutes one of the contra-indications to the use of the bath, according to Liebermeister, the frequency of internal hæmorrhage was materially diminished under the cold water treatment. Perforation is another contra-indication, but not

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[a] Cyclopædia of Practical Medicine. Ziemssen.

menstruation, except when there is no danger in the case. Pneumonia and hypostatic troubles offer no reason for suspending the baths.

The results of treatment are stated to be even more favourable than shown by figures, as the deaths occurred to a great degree among those for whom no treatment could have done anything; and it is further stated by Liebermeister that the entire appearance and bearing of patients under the antipyretic treatment is such that the old picture of a typhoid fever patient is no longer seen. The attendants do their part gladly, although the taking of temperature and giving of baths adds immensely to their labours, because the good results are so manifest, and because many of the annoyances of former times are, for the most part, done away with—such as involuntary evacuations on the part of the patient, violent delirium, bed-sores, &c. Finally, the physician takes a very different degree of comfort and pleasure in his calling, and feels much more certain of his results.

Immediately after the bath the patient should have rest, be wrapped up in a dry sheet, and put to bed, lightly covered, and given a glass of wine. In dealing with very feeble persons, the bath may be gradually cooled down, as recommended by Ziemssen, beginning with about 93° and adding cold water gradually until the temperature is reduced to 72°, or below. Such baths should be of longer duration.

Whenever the temperature in the rectum reaches 103°, or in the axilla 102·2°, a cold bath should be given. In mild cases, and in the latter period of severe ones, the after-lowered condition of temperature is often maintained for several hours; but, under severe attacks, the original temperature is very nearly reached in two hours, when a repetition of the bath is demanded. Hence, the duration of the effect of the bath is, to a certain degree, a measure of the obstinacy of the fever. In some instances the number of baths required by a patient during his entire illness has exceeded two hundred; but, in the majority of instances, four to eight per diem will be found sufficient, with fifty to sixty in the aggregate, especially if antipyretic drugs are administered at the same time—such as quinine, the use of large doses of which obviates the necessity for the so frequent use of immersion. To adults 22 to 45 grains of the sulphate should be given within the space of an hour at most. The temperature falls materially; the frequency of the pulse diminishes, and does not again begin to rise for six to twelve hours afterwards.* An important contra-indication is found in the existence of a high degree of weakness of the heart's action.

In *The Practitioner* for November, Dr. M'Combie, Assistant-Physician, District Fever Hospital, Homerton, sums up the result of the use of the cold bath in that hospital as follows:—" The results of our experience of

* The value of quinine administered in one or two large doses, *when the temperature had about reached its lowest point each day*, is confirmed by recent American experience.

the use of the cold bath in enteric fever lead us to believe that it is a valuable agent for mitigating the severity of the symptoms and of retarding the rapidity of the morbid changes on which the symptoms depend. We only employ the remedy in severe cases—cases marked by delirium, restlessness, sleeplessness, rapid pulse, and high temperature. Of 28 such cases 4 have died. In 2 of them the treatment had not a fair trial, as 1 died on the third day, the other on the fifth day after admission. The deaths were caused by congestion of the lung, paralysis of the heart, and hæmorrhage." He goes on to say:—"German statistics are doubtful, because they apply to mild cases, and because the diagnosis of enteric fever is made on slender grounds, including under the term many cases which simply show an increase of temperature, with slight restlessness and loss of appetite—cases called at Homerton ' simple continued fever;' and they administer the bath in mild and severe cases. Experience at Homerton did not justify the belief that the cold bath will reduce the mortality from 20 to 3·5 per cent. In unskilled hands its use might, in Dr. M'Combie's opinion, hasten death; but he considered it a valuable addition to the therapeutics of the disease, but that other agents, under certain conditions are of greater value." [a]

On the other hand, according to Liebermeister, where the antipyretic treatment of typhoid fever has been carried out with proper system, it has yielded uncommonly good results; and, in proof of this statement, he quotes the following statistics from the records of the hospital at Basle:—

1. Under indifferent treatment.—Total treated, 1,718; died, 469; per centage of mortality, 27·3.

2. Incomplete antipyretic treatment.—Total treated, 982; died, 159; per centage of mortality, 16·2.

3. Under systematic treatment.—Total treated, 1,121; died, 92; per centage of mortality, 8·2.

Here quinine and digitalis supplemented the use of the cold bath as an antipyretic.

Jürgensen gives even more favourable statistics from the Hospital at Kiel, where the mortality had been reduced from 15 $\frac{4}{10}$ to 3 $\frac{1}{10}$ per cent., and the results are stated to be even more favourable than these figures, as the deaths occurred, to a great degree, among those for whom no treatment could have done any thing.

The New York *Medical Record* for November contains the latest con-

[a] M. Sée, Professor of Clinical Medicine at the Hotel Dieu, Paris, condemns the bath as not only a useless remedy, but as absolutely dangerous, and considers sponging with cold vinegar and water equally efficacious. Quinine is, according to him, *the* remedy—no agent, except alcohol, more effectually lowering the temperature of the body. Liebermeister seems to confirm this when he affirms that if he was forced to the unpleasant alternative of adopting only one agent, he would, in the majority of instances, choose quinine in preference to the cold bath.

tribution on the subject. At St. Francis Hospital, New York, the cold
bath appears to be systematically used for the treatment of typhoid
fever when the patients possess the "*average physical strength.*" Such
persons are immersed to the neck in water, having a temperature of
85° to 90° F., whenever the body temperature rises to 103·5° F., cold
water being then steadily added, until a bath having a temperature of
from 58° to 60° F. is obtained. After remaining in this from ten to
twenty minutes, the patient is removed, rubbed dry, enveloped in a
blanket, and put to bed, and an ounce of brandy administered to promote
reaction. The thermometer is used fifteen minutes after the patient has
been placed in bed. As a rule, the temperature is found to have been
reduced from 2½° to 4° F. If, during the employment of the bath,
symptoms of marked prostration supervene, the patient is immediately
taken out of the water, rubbed dry, and warmly covered in bed.

The character of the eruption had been found in New York to be a
pretty safe guide with reference to the endurance of the baths in any
given case. As a rule, when the eruption had been abundant, and of a
deep red colour, the reaction after the use of the bath had not been what
was desirable, and it was found necessary to have recourse to some other
form of antipyretic treatment. Dr. Edeboels concludes by saying:—
"The cold bath, quinine, and iodine, with close attention to diet, and the
exhibition of stimulants when indicated, constitute the main features in
the treatment of typhoid in St. Francis Hospital, the cold wet pack
being occasionally substituted for the cold bath when the latter is not
well borne."

In conclusion, it may be interesting to compare the results of the
ordinary mode of treatment in this country with the antipyretic treat-
ment as carried out in Germany and elsewhere, taking Liebermeister's
statistics as a basis of comparison. As we have already seen, he gives
27·3 as the percentage of mortality before the introduction of the com-
plete antipyretic treatment at Basle. This seems to be a very high
death-rate as compared with the figures below:—

London Fever Hospital, percentage of mortality	- 16·6 to 18·2
Troops, United Kingdom, 1874 ,,	- 15·2
Cases treated in Royal Infirmary, Dublin, 1871-2-3 -	13·8
Epidemic amongst troops, Mullingar, 1871 -	11·1
Average mortality, Cork-street Fever Hospital, 1875-6	10·6
Cases treated in Contagious Hospital, Portobello, 1876	7·7
Cases treated in Royal Infirmary, Dublin, 1874 -	6·9

In contrast to these figures we have the antipyretic treatment at
Homerton in severe cases showing a mortality of 14·3, and the complete
antipyretic treatment at Basle 8·2 per cent. In the latter case it is
probable that many simple fevers were included, as the percentage

approximates very closely to that yielded by the statistics of the Cork-street Fever Hospital, for simple and continued fevers comprised, viz., 7·6. It is somewhat curious that, under the system of blood-letting and stimulants pursued by Cheyne at the Hardwicke Hospital in all fevers admitted during the years 1816 and 1817, the percentage of mortality should have been only 7·0.

The foregoing table shows that the ordinary treatment of enteric fever, as taught in the Dublin School of Medicine, yields results which in many instances bear favourable comparison with those given by Continental physicians.

In 1801 Mr. Leigh, Assistant Surgeon, Armagh Militia, then quartered at Coleraine, recommended "sea bathing in cases of typhus fever." The Physicians-General in Ireland, Drs. Quin and Harvey, to whom he wrote on the subject for their opinion, replied that, in the scantiness of facts then before them, " such a practice was only admissible when there was very great excess of heat, burning dry skin, delirium, redness over the body, great irritation, without any local or organic determination to the head, lungs, or bowels, or the slightest rheumatic complaint. Upon the whole, the contra-indications are so strong and so numerous that we would rather consider it more prudent to abstain from the use of a remedy so doubtful, and if not advantageous, liable to produce the worst consequences." It is curious how history repeats itself, for, under the general term " typhus," was then included much enteric fever.

The VICE-PRESIDENT said they were all aware that the treatment of typhoid fever by cold immersion had not been very generally practised in Dublin. He confessed he was one of those who had a prejudice against it, and for many reasons. In the first place, in most cases of typhoid fever there were chest complications of some kind or other, most generally bronchitic. The modes of treatment which he had found most efficacious for these had been warmth to the skin, the object being to direct action from the viscera to the surface. Under the circumstances, quite irrespective of the exposure of the patient, he objected to immersion. Again, he was not sure that the treatment by cold baths, viewed philosophically, stood the test of inquiry. It was not the treatment of a cause but of a symptom. No doubt, if the temperature of the patient ran to 105° or 106° the reduction of it might be some alleviation of his distress; but would that tend to shorten the period of the disease, or lead in any degree to a cure? If the high temperature was the result of combustion of tissue, it was manifest that by the discharge of the superfluous heat from the body, something might be gained towards abridging the period of the pyrexia; but that should be done by a process that would not entail dangerous consequences; for example, sponging the exposed parts of the body—namely, the hands, face, and neck. He had

done this with tepid vinegar and water, with very beneficial results; evaporation from the surface being thus promoted without the danger of a tepid or cold bath. In Dublin, where the use of the cold bath had not prevailed, the mortality was less than in those cities in which it did.

Dr. Lyons said that nothing had been adduced to show that cold immersion checked, modified, or in any way altered the progress of the disease. The alterations of temperature recorded in the chart exhibited by Surgeon-Major Gore, showed that there were two periods when the patient was in imminent danger. Whether this was due to the complications of the disease, or to the bath he could not say; but, at all events, two periods were recorded at which the descent of temperature was to a point at which they all knew there was most imminent danger of peritonitis. It was difficult to fix the exact day of inception of a case of typhoid fever; but in the case given by Surgeon-Major Gore on what was specified as the fifteenth day, the patient's temperature after the bath sank to below 98°. Any typhoid patient who, after a bath on the fifteenth day of the disease, did not speedily show reaction from such a temperature, would be, he considered, in most imminent peril. They knew that cases of typhoid fever were fatal from intensity of the morbific process in general, quite regardless of any specific lesion in any particular organ, save the intestinal complication. The chart simply indicated that the practice of immersion, though the patient might have recovered from it, was attended with very great risk. On the twenty-fourth day the temperature was nearly at a maximum, which showed that, so far as the pyrexial condition of the patient was concerned, nothing had been done to quell the disease. Having alluded to the prolonged duration of some cases of enteric fever, Dr. Lyons said that the case in question was one of comparatively short duration; but before they could arrive at a satisfactory conclusion as to the effects of cold immersion, they should have a strictly rigid classification of the forms and kinds of the disease—for they differed very much from one another—that were subjected to the bath; and they should see how far the general duration of the disease was modified by this form of treatment; how far aught was gained in diminishing the period of existence of the disease; how far it prevented ulceration, hæmorrhage, and death from the severe process of sphacelus in the intestines, or from the blocking up of them by the formation of Peyer's patches. He (Dr. Lyons) had not practised cold immersion; but he had, with tolerably satisfactory results, practised tepid affusion from a watering-pot. This was always gratefully borne by the patient, and remarkably promoted superficial circulation, giving a good colour to the surface and stimulating the nervous system. After all, affusion was only a means of assistance to other treatment. In the very case under discussion, the patient had been subjected to other treatment besides the

bath. Again, temperature could also be brought down by the use of quinine and digitalis. On the whole, he did not think they had evidence before them that cold immersion would be a safe practice for general adoption in enteric fever. On the contrary, he thought it was attended with serious danger, while there was nothing before them to show that it would suffice as the sole curative agency in a case. They knew that fevers differed in gravity in particular cases, and that epidemics differed in a remarkable manner from each other.

Dr. Grimshaw said that he had had some little experience of the use of the cold bath in enteric fever, and he believed the practice to be so dangerous that he had determined not to pursue it further. With respect to the comparison of cases occurring at different times and places, he believed it to be impossible to make such comparisons, since epidemics differed so much. A mortality of 27 per cent. in typhoid fever appeared to him to be so serious that, unless the epidemic were a very peculiar one, he should conclude from it that the treatment had been excessively bad. Surgeon-Major Gore had mentioned that under the bath treatment bed-sores grew less frequent. His (Dr. Grimshaw's) experience was that bed-sores were very uncommon if the patients were carefully nursed. Cases with a temperature of 102° in the axilla, or 103° in the rectum, he would be disposed to leave alone. The cases in which he had tried the cold bath were cases in which the temperature had reached 105°; and in every case he had to remove the patients from the bath, because their vital powers appeared to be failing. He did not lose any patient, but the appearances after the use of the bath were so alarming that he made up his mind that he would not try it again. Dr. Grimshaw quoted a passage from Dr. Currie's "Medical Reports on the Effects of Water," &c. (5th Ed., 1814, Vol. I., p. 40), as to the "alarming" effects which the author had himself observed to have followed the employment of cold water in fever; and also referred to the results obtained at the Homerton Fever Hospital in the treatment of enteric fever by the cold bath (*Practitioner*, Nov., 1876), which he, Dr. Grimshaw, did not consider so strikingly favourable as Dr. M'Combie appeared to think.

Dr. Doyle observed that, within the last two years, he had, with good effect, employed cold, in the form of a local application to the affected part, in cases of pneumonia and pleuritis, and in two cases of typhoid fever. In the typhoid cases he applied a wet napkin or towel over part of the abdomen where tenderness was complained of, and over that applied a layer of flannel. In those cases the patients complained of little pain; there was no haemorrhage or diarrhoea, and there were no relapses; and the patients were quite well in two months. The cases of pneumonia and pleuritis, in which he employed the cold application, got on remarkably

well. During a scarlatina epidemic at Howth, in 1874, he used cold applications to the throats of patients and found that they liked it.

DR. MACSWINEY observed that, assuming high temperature in enteric fever and other diseases to be but a symptom of the morbid process which was in operation, the cold bath could not be used for any purpose save to reduce the temperature. Salicylic acid, quinine, and other agents, were also used for the same purpose. At present, rightly or wrongly, there was an impression that the keeping down of temperature was an important item in the treatment of disease, though he did not see the object of doing so if high temperature were a symptom which would recur again and again. The drift of to-night's paper seemed to be that cold immersion had a curative power. Had it any such power?

DR. KENDAL FRANKS drew attention to the statistics of the use of the cold bath in enteric fever in the hospitals of Lyons, compiled by Dr. Mayet. These showed that the mortality from this fever, during the years 1872-73, previous to the use of the cold bath, was over 26 per cent. In 1874, Brand's treatment was, for the first time, fully tested. The death-rate among 279 cases, treated by this means alone, was 9·31 per cent. Of the 433 cases treated on ordinary principles, without the use of the bath, 12·47 per cent. had a fatal issue. This difference was the more remarkable as the cold bath was reserved for the severer cases only. In 1875, 34 cases were treated by Brand's system (cases alone in which threatening symptoms appeared); the mortality was 17·64 per cent.; whilst of the 129 milder cases, not treated by the bath, the death-rate was 18·65 per cent. During the nine earlier months of 1876, 22 cases out of 104 were treated to the cold bath, with a death-rate of 18·18 per cent., whilst 18·29 of the remainder terminated fatally. Bearing in mind the class of cases invariably subjected to Brand's method of treating typhoid fever, these statistics of carefully recorded hospital cases are strongly in favour of the use of the cold bath.

DR. FINNY had never yet employed the cold bath. It appeared to him that one item of the treatment, as mentioned by Dr. Gore, was noteworthy. It had been stated that after each immersion an ounce of whiskey or brandy was given to the patient, so that six or eight immersions in the day would lead to the patient taking a large quantity of stimulants.

SURGEON-MAJOR GORE, in reply, said that the object of using the bath depended altogether on what was the cause of the increased temperature. The metamorphosis of tissue which occurred in typhoid fever was a cause of high temperature. German physicians held that the high

temperature in typhoid fever was the cause of some of the most unfavourable symptoms, including paralysis of the heart and brain; and one object, therefore, for the use of the bath was to keep off those symptoms. A late French writer objected altogether to the cold bath treatment. Liebermeister preferred quinine to the bath for the reduction of temperature. The number of cases given by Dr. Cheyne was 800, and the mortality was only 7 in 100. He (Dr. Gore) was of opinion that the ordinary treatment in typhoid cases led to as good results as the bath.

Dr. GRIMSHAW read a paper on "The Management of the Bowels in Enteric Fever." [It will be found at p. 127.]

The VICE-PRESIDENT said Dr. Grimshaw deserved their thanks for his thoroughly practical paper. Typhoid fever was, unfortunately, very prevalent in Dublin at present. He agreed with Dr. Grimshaw that the administration of saline purgatives was one of the greatest of evils. Constipation was, no doubt, to be overcome, but the best mode of dealing with it was to unload the lower bowel by means of an enema, and to accomplish the rest by means of diet. Blistering the ileo-cæcal region he had found to be of great value in the diarrhœa of typhoid. The remedy he had found most useful in preventing diarrhœa was 15 or 20 drops of dilute sulphuric acid, with 5 drops of laudanum to each dose. His general treatment consisted in the administration of half-drachm doses of sulphurous acid every third hour. Occasionally morsels of ice in the mouth were very useful. A most important matter in typhoid fever was the regulation of diet. As long as the temperature was above the normal standard, it was not safe to administer a particle of solid food, or anything except milk, arrowroot, or bread grated into beef-tea.

Dr. LYONS alluded to the difficulty of recognising typhoid fever in its earlier stages, and to the danger of saline purgatives injudiciously taken during its inception. In cases of doubt practitioners should bear in mind the nature of the prevailing epidemic. Purgatives by the mouth should, in his opinion, be tabooed in the treatment of enteric fever, unless after the most mature consideration. Enemata were most strongly to be recommended, especially as in many cases the lower intestine was obstructed. A decoction of camomile flowers, with the addition of half an ounce of turpentine and the yolk of an egg, had long been used for that purpose in the Hardwicke Hospital. He had frequently checked diarrhœa by means of an enema, because it removed matter that nature was struggling to get rid of; there being from eighteen to twenty inches of intestine covered with diseased points, over which everything had to pass. He had not the slightest hesitation in saying, from an experience of many thousands of cases at home and abroad, that it would be far

better to have no motions of the bowels in the day at all, after the prelimi-
nary diarrhœa, than to have three or four, as Dr. Grimshaw apparently
thought unobjectionable. There was a period in enteric fever at which
the lesion, not having passed into a condition of ulceration, remained in
a condition of quiescence, and, if let alone, could be gradually softened
away. Incautious purging, or the use of improper food, such as chops,
chicken, &c., during the period of partial convalescence, was almost
certain to light up the mischief again, and lead to a fatal issue. Milk,
eggs, arrowroot, lime-water, and Carrara-water, were quite sufficient to
conduct the patient over a very long period of the disease. He had seen
a case end fatally after a duration of three months, with a typhoid lesion
in a condition of extreme activity, in consequence of the injudicious use
of purgative medicine ; and in another case also of long duration a fatal
issue likewise resulted in consequence of the use of solid food. Turpen-
tine he regarded as a most valuable medicine; but his experience of
blisters was not favourable. Hæmorrhage, which was a sudden and
treacherous complication, was, in his opinion, most likely to occur in
individuals of a thin, delicate, strumous habit of body ; it was in cases
of that kind that he had met with the most sudden and uncontrollable
hæmorrhage.

Dr. DOYLE remarked that Niemeyer recommended five-grain doses
of calomel. He (Dr. Doyle) had used calomel in many suspected cases
of typhoid fever; and, where the disease set in with constipation, he
had used calomel with good effect, as it acted by liquifying the fæcal
matter.

Dr. DUFFEY called attention to a recent article on "The Intestinal
Lesions of Typhoid Fever," in *The Philadelphia Medical Times*, an
abstract of which was given in the last (January) number of THE
DUBLIN JOURNAL OF MEDICAL SCIENCE, p. 110. It was most desirable
that the laxative used in cases of typhoid should be one which caused
the least possible amount of peristaltic action in the intestinal canal.
Dr. Henry, the writer of the article, condemned castor-oil as a purga-
tive in these cases, on account of its power of exciting peristalsis, and
yet it was the purgative almost invariably used in typhoid, probably on
account of its known physical properties as a lubricator, and hence as
soothing to inflamed mucous membranes. Another class of purgatives,
the salines, caused a watery secretion from the mucous membrane.
These had been universally condemned in the course of the present
discussion, especially the sulphate of magnesia, although the saline purga-
tives did not, it was stated, increase peristalsis.

Dr. JAMES LITTLE said he was satisfied that the great object in
the management of the bowels should be to avoid interference in the

-directions either of astringent or laxative treatment, unless it was abso-
lutely necessary. The rarer cases for interference were those calling for
purgatives. In most cases of enteric fever which threatened to be fatal
the danger was threatened in some particular direction. At the present
time there was a tendency to cerebro-spinal complications. In every
case they had had in the Adelaide Hospital for the last few weeks, there
had been severe pains in the back of the head and other indications
of cerebro-spinal complications. In cases where the head was much
threatened purgatives were, he thought, sometimes required. Many
years ago, during a winter spent in Rome, he saw several typhoid cases;
and from conversations with Dr. Vellini, an able physician, was then im-
pressed with the necessity of allowing purging to go on, and of producing
it if it did not occur, in consequence of the liability to head complications.
Where the patient in the early stage of enteric fever had severe headache
and great heat of head, purgatives, particularly calomel, were useful.
Before the ninth day the patient could often be relieved of his headache
by a five-grain dose of calomel. Later in the fever, when the abdomen
was tympanitic or distended, he had given a teaspoonful of castor-oil and
turpentine. The position of fæcal accumulations could be ascertained by
percussion of the abdomen; and if the accumulation was in the descend-
ing colon, an enema would relieve it. But if there was a general dull
condition of the whole abdomen, an enema would not act so satisfactorily.
There was a class of cases in which typhoid fever attacked young women
who had habitually neglected their bowels long before the fever com-
menced; and it had been thought that the decomposition of retained
fæcal matter in the bowels gave rise to a poisonous condition of the
system. He lately saw, in consultation, a young lady who had been four
or five weeks ill of typhoid fever under circumstances of that kind. On
examining her he found that she had dulness on percussion, and a dis-
tended condition of the whole transverse colon. He suggested a tolerably
large dose of castor-oil, and the gentleman who attended her afterwards
told him that he had repeated the dose. As to diarrhœa, if the cause of it
could be removed, it was better than using astringents. Sulphurous acid,
according to his experience, would prevent diarrhœa from setting in,
probably arresting decomposition, but once it had set in, it would not
arrest it. Of all the remedies that he had heard of for controlling
diarrhœa in typhoid fever, the best was one for which he was indebted
to Dr. Hudson, and which, he thought, was infinitely superior to any
other astringent in respect of safety and general satisfactory character.
It consisted of a pill composed of a sixth of a grain of opium, the same
proportion of carbolic acid, and three grains of bismuth. He had found
the application of two or three leeches over the ileo-cæcal region to be of
benefit, and blistering was not unfrequently followed by mitigation of
bowel irritation.

DR. GRIMSHAW, in reply, said that the bowels might be left for forty-eight hours without being moved at all, and that if the bowels of a patient were moved four times in the twenty-four hours he would not check it. He did not think blisters useful unless when there was a tendency to diarrhœa after the fever had subsided, and then blisters over the right iliac region were useful. He sometimes used calomel. He believed salines were objectionable, and therefore did not give them. He constantly employed enemata for unloading the lower part of the bowel, but they were of very little use if the accumulation was in the transverse colon. As to sudden death from hæmorrhage, he never had had such a case in his practice. He believed that in the majority of such cases the hæmorrhage had occurred in consequence of the patient being allowed to get up and strain at stool, when he never should have been suffered to leave his bed. He had also used sulphurous acid, not to control, but to prevent diarrhœa. Since the last meeting he had tried salicylic acid, and he believed that it would be found more useful than sulphurous acid in preventing diarrhœa.

The Society then adjourned.

APPLICATION FOR BURNS.

A GOOD remedy for burns should completely protect the part from contact with air, by forming over it a flexible pellicle. The preparations most frequently employed with this object hitherto have been Carron oil, collodion, and Buck's mixture (mucilage of gum arabic, mucilage of tragacanth, and treacle sufficient to make it of the consistence of honey). Rice proposes a formula which fulfils all the necessary conditions:—440 grammes of strong clear glue, previously broken into small pieces, are softened in a litre of water, the solution being afterwards completed by means of a water-bath; 60 grammes of glycerine and 22 grammes of carbolic acid are added. The evaporation is then continued till a brilliant pellicle forms on the surface. On cooling, the mixture forms an elastic mass, which must be liquified by heat when required for use. It is applied by means of a large camel's-hair brush, and in less than two minutes a brilliant, flexible, and almost transparent coating is obtained.—*Apotheker Zeitung & Journ. Alsace Lorraine,* Dec. 28th, 1876.

K. F.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-NINTH ANNUAL SESSION.

Saturday, December 9th, 1876.

THOMAS DARBY, F.R.C.S.I., President, in the Chair.

DR. L. ATTHILL exhibited a specimen of a very large myxomatous tumour of a benign nature, which he had removed from a patient still in the Rotunda Hospital. The patient was a young, healthy, married woman. The tumour appeared about twelve months before her admission to hospital. It was then about the size of a walnut; at that time she was pregnant. After parturition, it partially disappeared; after a lapse of two or three months, however, it increased rapidly in size, and, although painless, caused so much inconvenience, that she wished to have it removed. Under chloroform, and after a troublesome operation which occupied half an hour, this was effected; it proved to be of enormous size, and so deeply seated that its enucleation was a dangerous and difficult matter—the danger of hæmorrhage compelling him to use only his fingers and the handle of the knife. A very large cavity was left, which extended down to near the tuber ischii; this was filled with dry cotton. During the operation there was but little hæmorrhage; but after three days, sharp secondary hæmorrhage occurred, which was checked by the introduction of cotton saturated with perchloride of iron. The labium was still thick and indurated, and Dr. Atthill believed the patient would soon return to have it removed. The tumour was enclosed in a very indistinct capsule.

Notes on Diphtheria. By HENRY KENNEDY, M.B.; Physician to Simpson's Hospital, and the Whitworth, Drumcondra.

To the following paper I have given the title of "Notes," because it does not profess to be, in any way, a general account of the disease of which it speaks. It is in fact a few jottings down of diphtheria, as it has come under my own immediate notice, within the last few years. Amongst these jottings, however, one question has cropped up which I am particularly anxious to bring before the members of the Obstetrical Society—I mean the identity or non-identity of diphtheria and croup—a question which, as you are aware, has been widely discussed in London and elsewhere very lately, and which would seem to have ended in

the opinion that the two affections are identical. I shall reserve what I
have to say on this point till further on in these remarks. It will be in
the recollection of some present that several years since a rumour began
to prevail that diphtheria had.appeared in England, and some time sub-
sequently it was found to have come amongst ourselves. It may be
observed, in passing, that this is not the first time disease or rather
diseases broke out in England, and then spread, or seemed to spread, to
this country. I do not say it was contagion, as ordinarily understood,
which brought these diseases, but simply that they spread; their existence
in England having gone before their presence amongst ourselves. In this
way it was that, in the year 1832, cholera appeared first in England, and
subsequently here; and again, in the great epidemic fever of 1847-48—
while Ireland got all the credit of generating the fever, and the terms
"famine fever," "Irish fever," and so on, were very common in many of
the English newspapers, the real facts were, that for many weeks before
fever prevailed amongst us here, it had existed in many parts of England
far above the averages. I mention this point especially, because I have
reason to know much misunderstanding has existed about it. Just in the
same way as already stated, diphtheria prevailed in England for a con-
siderable period before it reached this country. The first case which
occurred to myself was as follows, and I must say came upon me some-
what by surprise:—

CASE I.—A large fat woman, of forty years of age, was admitted into
the Cork-street Hospital, labouring under typhus fever in a very severe
form. She was densely spotted, the brain being much engaged, and had
now been nine days ill. On my visit this day I could not but observe
that a very marked change had occurred from the day before. Her
countenance had become brighter, whilst the nurse reported that her
power of swallowing was not at all as good as it had been. These were
contradictory symptoms, if I may so speak of them. For when any-
thing of dysphagia shows itself in typhus, it usually goes hand-in-hand
with the brain getting more and more profoundly engaged. Here, how-
ever, it was otherwise. But the symptom which had specially changed
from the previous day was the pulse; from being one of 130, and each
beat running into the other, it had now fallen some ten beats, each being
distinct, whilst its character was full and bounding. The heat of skin, too,
had increased. In fact the type of the fever had materially altered; and, on
closer examination, I found that one side of the neck was swollen, and on
looking into the throat I observed that part of the fauces corresponding
to the outside swelling was covered with a whitish pellicle. Diphtheria,
in fact, existed, and this at the very time the fever was at the worst.
It is enough to say of this case that she was treated mainly with barm
and wine, and that after a very severe struggle she recovered, and more

quickly than I could have anticipated. Subsequently to this period I met a considerable number of cases of a similar kind—that is, where diphtheria suddenly occurred in the progress of fever, both of the typhus and typhoid type. In Murchison's able work the writer alludes to the occurrence of diphtheria in typhoid alone, but it is certainly met in typhus also, of which a very marked example was under my care within the last two months. As the result of my entire experience on this point, I may add that the occurrence of diphtheria in the course of ordinary fevers is not by any means a fatal complication. It is quite true I have met fatal cases and had them under my own care, but, considering the characters of the two diseases, I repeat it is not a very fatal coalition. It has, indeed, appeared to me, and more than once, that the attack of diphtheria, or rather the change in the character of the fever, and to which allusion has been already made, was beneficial rather than other-wise. But I cannot pursue this subject further here. Besides common fevers, it need scarcely be stated that diphtheria frequently complicates scarlatina, and it would seem as if there were some very close connexion between the two. I have often seen some members of a family attacked simultaneously with the two affections; while others, sick at the same time, only exhibited scarlatina. I may mention, in connexion with this subject, that many years since, when scarlatina prevailed most extensively in Dublin, and when the formidable swellings of the neck were infinitely more frequent than at the present time, it was often the practice to blister the swellings with the hope of lessening them. Under these cir-cumstances I frequently observed the blistered surface exude a pellicle of lymph, which I now know was diphtheric, but which I did not recognise at the time. It need scarcely be added that the practice to which I allude I would not now follow. Others, I believe, have spoken of the same result from blisters.

Besides the distinct co-existence of the two diseases, of which one was diphtheria, I have seen this affection attended by symptoms which are worth a passing notice. Thus I cannot doubt having seen, and on different occasions, cases where, from first to last, none of the special exu-dation, said to be characteristic of the disease, showed itself. But why, it may be asked, did I think so? Simply because other cases in the same house, and at the same time, had the regular disease, and because the cases which showed no exudation were as long in getting well as the others. This absence of exudation has, I know, led to difficulties in the diagnosis, possibly even to the overlooking of the disease. Just as in scarlatina the disease may run its course without the ordinary eruption, or the usual sore throat, so in diphtheria there may be no exudation in the throat. In the following case I confess I was in great doubts of the nature of the case when I first saw it. But it was soon afterwards made clear enough.

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CASE II.—I was asked by Dr. Wyse to see a child of six years of age, who had been unwell for about a fortnight, but had received little or no attention from her friends. In fact, the illness had not been so severe as to make the child take to its bed. There was some obscure history of the attack having commenced with sore throat. But inspection showed little or nothing to help the diagnosis. The symptom, however, for which our advice had been asked was a difficulty in swallowing. With this it was noticed that the speech was much impeded, and though the child had evidently the wish both to speak and put out its tongue, it could do neither; and a very strange appearance, I must say, it gave the child. It need only be stated further that the child died within three days of this time, and the character of the disease would have remained unknown, or at least have been only guessed at, had not another child in the same house been now attacked with diphtheria in a well-marked form. It may, I think, be reasonably concluded that the first child died of the effects of the same disease. Speaking of the symptoms which may attend the disease, there is one worth noticing, and about which I am not sure much or any notice has been taken—I mean epistaxis. In one instance it occurred to an alarming extent, and it was chiefly on account of it I was sent for. Parties who had seen the child before me had spoken doubtfully of the nature of the case, and when the bleeding occurred, as just stated, I saw the patient. The loss of blood must have been considerable, judging by the countenance, whilst the pulse was very rapid. My attention was at once caught by some swelling on one side of the neck, and this, coupled with the high fever, led me to look into the throat, where there was no difficulty in seeing the characteristic exudation of the disease, which had formed on one side of the fauces. The patient, I should have said, was a girl of eleven years of age, and made a very tedious recovery, which, however, may have been accounted for by the fact that, as the throat got better, the disease showed itself on the vulva—fortunately, however, not in a severe degree.

Of the various parts on which the exudation may show itself, the internal fauces take, of course, the first place, and from this it may spread in any direction, and, as is well known, ultimately affect the hearing, the smelling, the voice, rendering it nasal, or causing the child to snore in its sleep, of which last I very recently had an example under my charge.[a] In rarer cases the exudation shows itself on other parts. Thus, in a case which I saw with Dr. Barker, it was very distinct on the lips and

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[a] In connexion with this part of my subject, allusion may be made to a case brought before the Pathological Society of Dublin several years ago. The case was that of a child who died of diarrhœa and wasting, and on *post mortem* examination an exudation of lymph was found to coat, and be adherent to, the entire mucous intestinal tract, beginning in the stomach itself, and reaching to the rectum. Not an inch of the whole canal was free of it. Whether this coating was of the diphtheric kind, I shall not take on me to determine.

nostrils, and there was even an appearance of it on the conjunctiva of one eye. When I speak of the special organs being affected, it might be more correct to describe them as after-effects, for it is well known they frequently make their appearance long after the exudation has gone. Nothing, it seems to me, could be more convincing that the disease of which I am speaking is a constitutional one, than the fact of two or more parts being attacked at once, or it might be in succession; and this is also shown by the well-known fact of the renewal of the exudation in the throat itself, and this may occur again and again. This, it seems to me, is a point we should never forget. I have purposely omitted till now one of the complications which may arise in the course of the disease—I mean the exudation passing down into the larynx and trachea, and so causing croup, or a disease like it. It has been already stated, and you are all aware, that within a very brief period a great discussion has taken place in London as to the identity of this form of croup—that is, what may be called diphtheric croup, and the croup described years since by Cheyne and other authors—and the conclusion which seems to have been arrived at is, that the two affections are identical. In this view I cannot coincide. I believe the two diseases to be essentially distinct, and for the following reasons :—Cheyne's croup, as it may be called for the sake of distinction, has always appeared to me to be a local inflammation of the larynx and trachea, and more rarely of the bronchial tubes; and this inflammation is followed by the effusion of lymph on the mucous surface. In this disease it has never been my lot to see the inner throat implicated. On the contrary, when inspected, it has been singularly free from disease. What does precede the attack of the larynx is bronchitis, and this often exists a few days previously. In fact, the attack begins in the lung, and then spreads upwards to the trachea, and so to the larynx. This state, too, is accompanied with fever of a much more sthenic character than what I have seen with diphtheria, and I need scarcely add, as bearing out this view, that the treatment which answers best in the first stage of the two affections is directly opposite. The one is some modification of the antiphlogistic treatment, while the other is the very contrary. I have never seen any case of diphtheria where I could have ventured on the use of tartar emetic, but I have nearly always done so in cases of croup. But further, I have seen nothing which would lead me to think that Cheyne's croup was a contagious disease, whilst I have with diphtheria; and all know that two, three, and even four members of one family, and at the same time, are often attacked.

But again, there is a symptom described which I have often seen, but only in diphtheria—I mean an enlarged gland or glands, and it is curious how frequently it is confined to one side of the neck. I have never seen this in croup. It is but right to observe, however, that it is not always present in diphtheria.

But still further—anyone who has watched cases of both diseases, which are going on to a fatal termination, cannot fail to have been struck with the marked contrasts they afford. In the croup it is a still-increasing dyspnœa, most painful to witness; whilst the jugulars are full, the face is pale and swollen, the lips blue, and the expression that of anguish. In diphtheria, on the contrary, the face exhibits none of these features; it is sunk, and, as it were, resigned. There is good reason to conclude that the suffering is next to none, and the death comes by asthenia, and in its best-marked form. It is difficult, to my mind, to speak of a greater contrast than these two states afford. But there is still another reason which seems to me to separate these two affections. Croup, we know, is a disease of childhood, and attended by exudation on the mucous surface. Now this very rarely occurs in adult life, and when inflammation does attack the larynx in grown people, we know the result is œdema of the glottis, of which disease the celebrated Washington died.

To all this line of argument I know it will be said that I have omitted to notice those cases of diphtheria where the exudation spread from the fauces to the larynx, and so caused croup, which it is not possible to distinguish from the other forms. Now, whilst admitting that the disease so spreads, it does not by any means seem to me to follow that the two affections are identical. There is no reason why the larynx should not exhibit varieties of disease, just as the throat or the stomach do; and though there is exudation in both the affections under consideration, and on the very same parts, it is not a necessity that it is of the very same character in each. In the cases of fatal diphtheria exhibited at our Pathological Society—and there have been a good many—I did not see one where the rima glottidis exhibited the appearances of genuine croup, such as may be seen by any one in the Museum of the College of Surgeons. Effusion of lymph there undoubtedly was in some, but it was semi-fluid, and in detached portions. Nor has it been my lot to see any case die from direct obstruction of the rima, nor any case where diphtheria began in the larynx. Yet I have had my share of fatal cases, and in one family no less than four children were swept off, all of them dying in an asthenic state, and the youngest dying first. When the breathing was oppressed it has not seemed to me to be due to obstruction at the rima, but to that general state of the system which so constantly goes hand-in-hand with all those acute diseases in which the constitution is profoundly engaged.

For these several reasons, then, I must side with those who consider the two affections distinct; and, at the risk of being thought tedious, would repeat the several points already given:—

1. That diphtheria begins in the throat, and from this may spread downwards, so as to involve the larynx; whilst Cheyne's croup either

commences in the larynx, or as bronchitis, and then spreads upwards to the larynx.

2. That diphtheria is essentially a constitutional affection, whilst there are no grounds for supposing that croup is of the same character.

3. That whilst the fever of croup is very commonly sthenic or inflammatory, that attendant on diphtheria is in a very marked degree asthenic.

4. That the treatment of the two affections is, in consequence, essentially different.

5. That diphtheria very generally exhibits some enlargement of a gland or glands in the neck—a state which is not observed in croup.

6. That the progress of the two affections towards a fatal result affords a very marked contrast the one to the other.

7. That there are no grounds whatever for supposing Cheyne's croup to be contagious, whilst there are the strongest for supposing diphtheria to be so.

As bearing on this point of my subject, I may say that, very recently, I have looked into Cheyne's work, and find it expressly stated that inspection of the inner throat showed it was not involved in the disease he described; and his father's experience, which seems to have been of the very largest, was to the same purport.* If this be not strong proof in favour of the views I would advocate—viz., that the two affections are distinct—I know not what is, for Cheyne had no theory to support, and is acknowledged by all to have been a most accurate observer as well as describer of disease.

This question is not one either of mere curiosity. It has a most important bearing on the treatment of these affections, and on no point more so than the question of operation, when such proceeding may be required. But it would be quite foreign to my present purpose to enter on this question here.

In the preceding observations some of the points bearing on the diagnosis of diphtheria have been noticed, and I would call attention to it again, not because the disease, when once seen, can be mistaken, but because it may be overlooked, even when present. It is by no means uncommon to meet cases where the child, or even adults, make no complaint of their throats, or the patient may be so young as not to be able to express its suffering. In the former the disease may readily be overlooked; and for myself, I believe the disease, in many instances at least, does not entail that suffering which might be expected. The sensibility of the parts, so far from being increased, has seemed to me to be diminished, and hence less complaint is made; and, even when dysphagia exists, I have thought it was more due to a want of power than to the presence of pain; nor,

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* On referring to Houston's Catalogue of the College of Surgeons' Museum, I find that, out of nearly twenty specimens of croup, two only are described as diphtheritic, whilst in most of them the inner throat is expressly stated to have been quite healthy.

from what I have seen, would I compare the suffering caused by diphtheria with that arising from other affections of the throat. This point, I think, is one of some moment, and I should be glad to hear the opinions of others on it. The age of the patient is, as already stated, another cause for the possibility of overlooking the disease. Within the last month a case of this kind came under my notice. A mother applied at the Whitworth Hospital, Drumcondra, complaining of sore throat. On inspecting the throat, it exhibited all the characters of diphtheria; and with it she also directed my attention to her right ear, which presented the signs of erysipelas, being much swollen and red. This was a complication I had not seen before. The woman was carrying an infant of four months old, and she casually stated that she thought it was not well; but, though nursing it, she did not say a word of any inability to swallow. The idea, however, at once struck me that, possibly, the infant had the same disease as its mother, and on looking into the throat, the idea was confirmed, for diphtheria was well marked. Contrary to what I expected, this infant has recovered in a degree, but is not yet quite well. On the treatment of diphtheria I have very little to say. To the inner throat I believe it is always well to apply some kind of caustic, either the solid nitrate of silver, or a solution of it. I have found the tincture of perchloride of iron answer well. It may be used very freely, and is sufficiently caustic to act as such; or, when the fœtor is great, the glycerine of carbolic acid suits admirably. I know not whether the experience of others has been the same as mine, but poultices I have found of little or no use. They do not give that relief which is afforded in other inflammatory affections of the throat; and this, in a degree, bears out what I have already stated about the diminished sensibility which seems to pervade those parts where exudation exists. The general treatment which suits best is what may be called a tonic one, including both liquid food and wine, the mineral acids, with preparations of iron, and sometimes, but not always, the chlorate of potash. On this part of my subject there is only one point to notice. In several cases I was unable to get the throat to heal; and it may be observed, in passing, that it is commonly a much longer process than which might be expected—the redness and excoriation, if there be not ulceration, persists long after the patient ceases to complain. Neither is it by any means uncommon, as is well known, for the exudation to repeat itself again and again. Under these circumstances, the treatment which has been just glanced at has often failed, and I then had recourse to Fowler's solution, and with very satisfactory results. Indeed, I have sometimes thought that this medicine might have been used with advantage at an earlier stage of the disease.

The PRESIDENT.—This is a paper of great interest and of great practical importance. The disease known in this country as Cheyne's

croup, was confined to the interior of the larynx and trachea, spreading down to the third, fourth, fifth, and sixth subdivisions of the bronchi, but nothing of diphtheria, under that name, was known here until of late years. Dr. Kennedy has omitted one point—namely, the paralysis that sometimes follows diphtheria. Children have not unfrequently become paralysed by it, and rendered unable to walk for months; and this sequel alone is sufficient to make a great distinction between it and inflammatory croup, which I have never known or heard of being followed by paralysis.

Dr. MacSwiney.—Dr. Kennedy, in his paper, has courageously and characteristically rushed in to defend the weaker side. It is not the first occasion on which I have known him to do battle for the losing cause. It has, however, become apparent now that we must modify our former views respecting " croup ; " and there is, in fact, a great preponderance of opinion at present to the effect that Dr. Cheyne, and those who held the views of his time, were wrong in their opinions as to the nature of that disease, and that croup—that is, pseudo-membranous croup—and tracheal diphtheria are identical diseases. In this opinion I entirely concur. I must say that Dr. Kennedy, by the use, at the commencement of his paper, of the single word " croup," somewhat obscured the question. Accordingly, to my experience, we find the name " croup " often used to designate three totally different affections—namely, spasmodic croup, or laryngismus stridulus, the ordinary inflammatory laryngitis, and tracheal diphtheria. Certainly in English medical literature these three affections have often been confounded under the name of croup. But, strictly, " croup " is not so much a disease as a name of about a hundred years old, which, obviously, was given to the affection on account of the stridulous sound connected with the malady. It originated in Scotland; to this day, in that country, a person is said to be " croupy " when he has a hoarse stridulous cough. I believe the first notice of croup in our medical literature is in the writings of Meade, who received from a Scotch physician, named Home, an account of an affection which the latter called " croup," and the symptoms of which were those of inflammatory laryngitis. It may be interesting to recall to our recollections what Bretonneau of Tours, who, in 1818, first made us acquainted with epidemic diphtheria, thought of it. In England we have the authority of Sir Thomas Watson and others to show that we were practically unacquainted, in modern times, with epidemic croup or diphtheria, in the form in which it occurred in Tours, until 1857. In that year there was a great epidemic of diphtheria, and it was the first we ever heard of in this country. Sir Thomas Watson states to the effect that, during a practice of many years, he had never seen a case of diphtheria, as such, until 1857. Bretonneau, in his description of the epidemic at Tours,

states that that disease was the "croup" of Home (the same who corres-
ponded with Meade), and that it occasionally attacked the larynx and
trachea alone, being strictly confined to that region.   But it is not in
England alone that the opinion has been gaining ground that pseudo-
membranous croup and laryngeal diphtheria are, in fact, the same disease.
In France, Velpeau, Trousseau, Guernsant, and others, taught this
doctrine long since; and I think the majority of physicians there speak
now of "croup" and "vrai-croup."   By the former they understand
ordinary inflammatory laryngitis, and spasmodic or "nervous" croup,
called laryngismus stridulus here, whilst under the name of "vrai-croup"
they designate what we call tracheal diphtheria.   A great epidemic of
diphtheria took place in Florence about six or eight years ago, and a
report on the subject was made by the Florentine Medical Society, in
which they declared the universal medical opinion of Italy to be that
pseudo-membranous croup and tracheal diphtheria were the same disease.
In Germany it is so also.   I do not here speak of the well-known pathology
of Virchow, in which he denotes a croupous and a diphtheritic exudation
according as it is superficial or deep—following whom many German
physicians speak of "croupous," applying it to the superficial, and
"diphtheritic," applying it to the deeper exudations on the mucous mem-
brane.   But I believe I am correct in saying that the great body of the
German physicians now hold that, clinically, those two affections are
identical.   Thus, in England, France, Italy, and Germany, we have a
general *consensus* as to the identity of those two diseases.   But they are
most careful, when they speak of "croup," to indicate that they mean the
affection with the pseudo-membranous exudation on the mucous mem-
brane.   It is denied that there is any such exudation from inflammatory
laryngitis; and the advocates of the identity of inflammatory laryngitis,
with true exudative croup, are challenged to adduce a single example of
inflammatory exudation the result of inflammation of the mucous mem-
brane of the larynx.   Sir William Jenner, whom I think I may claim as an
advocate of the identity of those affections, asserts that no anatomist could
discover the smallest difference or distinction between the membranous
exudation of diphtheria and the exudation of exudative "croup."
Nevertheless, Dr. Kennedy has manfully sustained the opposite view, in
which he is, no doubt, fortified by the statement of Dr. West, a distin-
guished authority, who is still inclined to hold that they are two distinct
affections.   In a late discussion on the subject in the Medico-Chirurgical
Society, Dr. West declared that "the last word on diphtheria and croup
had not yet been said."   Dr. Kennedy has verified that by his admirable
paper of to-night, which—apart from the differences of opinion as to the
ultimate nature of the affections—contains practical observations that are,
in my judgment, of great weight and value.

The PRESIDENT.—Dr. MacSwiney has spoken of pseudo-membranous croup and laryngismus as being the same as inflammatory croup. As I understood Dr. Kennedy's paper, he did not allude to that peculiar form of disease. He spoke of the croup we have seen over and over again, where, in those who have died, a strong leathery lining is found in the trachea, extending into the bronchial tubes. I have, myself, seen many cases of this where the rimæ and the fauces were not affected at all, but where the disease was entirely confined to the vocal cords—the trachea and larger bronchi. We have even seen this thick leathery substance vomited up. True, croup is a sort of endemic disease, found in certain marshy localities, not so with diphtheria. What has been commonly called "thrush" and "malignant sore throat" in this country, is probably closely allied to, if not identical with what we now call diphtheria, from an attack of which neither age nor locality is a protection; whereas Cheyne's croup has not been found to attack children after the age of fourteen or fifteen years, and under that age the same individual has been known to suffer from repeated attacks. I remember attending a spare delicate child, of six or seven years old, many years ago in his fifth attack of croup. Bleeding in severe cases was the approved practice in those days, but I hesitated to resort to it, when the child cried out—"You must bleed me in neck; Dr. Nolan bled me in the neck the last time." Upon that appeal from the little sufferer, I opened the jugular vein, and took off a couple of ounces of blood with the best effect. That child is now one of the strongest men of my acquaintance. I have frequently seen leeches applied; and in one case, six were applied to a child of eighteen months old, and, as I believe, saved its life. That is a kind of treatment you would not propose for a diphtheritic patient.

DR. MONTGOMERY.—There is an interesting point in Dr. Kennedy's paper on which I have had some little experience. He alluded to the connexion between diphtheria and typhoid fever. A member of my family, a boy, eleven years of age, had a very severe attack of typhoid fever. After about three weeks diphtheria developed itself, with the usual ashy grey coating on the pharynx, &c., and caused much anxiety to the physician who kindly watched the case. Under the usual supporting treatment, combined with the administration of hyposulphites and the liquid extract of bark, he improved. But I remember it was after the more severe symptoms of diphtheria began to subside that the fever yielded somewhat quickly. He had aphonia for a long time, with indistinct pronunciation, and his recovery was very slow. The preparation of extract. cinchon. flav. liquid., I think, contains the best qualities of the bark in a concentrated form, and in this case was most advantageous.

DR. JOHN A. BYRNE said that as far as his experience went, he did

not think that croup and diphtheria were identical diseases. Dr. Kennedy
had alluded to one circumstance, upon which he laid stress, as being
a valuable diagnostic between them—viz., the fact that in croup seldom
more than one of the family is attacked, whereas in diphtheria many of
the same family may be. Now a few years since he had met with
a case, which certainly did not bear out the statement made by Dr.
Kennedy. A fine boy, aged ten years, was attacked by croup. He
was attended by Dr. Hayden, Mr. Hayes, and by him (Dr. Byrne).
Tracheotomy was performed by Mr. Hayes, but the boy died. In a
few days his sister, a child of three years, was attacked and died. Now
in neither of these cases was there the least appearance of diphtheria,
nor of scarlatina; and there were not any cases in the neighbourhood as
far as he could ascertain. The house in which the family resided was
a fine healthy one, and did not appear to be deficient in sanitary arrange
ments in any respect. He had seen many cases of croup, but he did not
remember seeing one which was succeeded by another so rapidly occur-
ring in a number of the same family.

DR. MACSWINEY.—Dr. Kennedy has asked for my own individual ex-
perience on this subject. It is rather small, but altogether tends in a
direction opposed to Dr. Kennedy's views; and from it, together with the
best attention I have been able to give to the matter, I think the two
affections which I have specified are identical. I shall just notice one
circumstance which has not been adverted to by Dr. Kennedy. Diphtheria
is an affection that may be found in any part of the mucous membrane,
the throat included. It may creep from the throat to the larynx and the
trachea, giving the symptoms of croup; but, on the other hand, it may
affect the mucous membrane of the trachea and the larynx alone, and no
other part of the body. In true laryngo-tracheal diphtheria you could
see nothing upon looking into the throat, because the disease is confined
to the trachea, and extends nowhere else; numerous dissections have
shown such to be the case; and Bretonneau and others refer to many
such examples. The question, then, that you have got to answer is—are
there two true croups, very inflammatory laryngeal croup and pseudo-
membranous croup?

The PRESIDENT.—I should say there are two, as far as my experience
goes.

DR. KENNEDY, in reply, said:—I am glad that the paper has given rise
to so much discussion. The subject of paralysis, as a result of diphtheria,
to which you, Mr. President, alluded, came under my notice but once,
and the case has been already given. The paralysis, no doubt, affects
various joints of the body. I should have been glad if Dr. MacSwiney

had given us the result of his own experience as to whether the two diseases, of which I have said so much, are identical. I may state Cheyne's work came out in 1809, and I must repeat that when his own and his father's experience are taken together—and we find them stating that the internal throat was quite free—I cannot imagine anything I could advance would afford a stronger argument in favour of the views I have advanced. As to Dr. Montgomery's remark about the use of the extract of bark, I may state it was a favourite remedy a long time ago, and it can be made into a very palatable mixture, even for children. It was used long before quinine was discovered. In reference to operation—a point to which Dr. Byrne alluded—I must speak with great reserve. It is certain it has succeeded, and in a good many cases where such a result could scarcely have been expected. Trousseau seems to have been very successful. It is obvious that every case must be considered on its own merits; and whether it be diphtheria or croup, if anything of operation be tried, it would be done as early as possible after certain symptoms have declared themselves. Speaking from what I myself have seen, I would say croup was a more favourable disease for operation than diphtheria.

Epithelioma of Cervix Uteri—Operation ; Death from Collapse.—DR. ATTHILL said : I have some diffidence in showing this specimen to the Society, nevertheless I consider it to be my duty to do so, for we ought to publish our failures as well as our successes. The patient from whom I removed this growth was sent to me from a remote part of the country, evidently fast sinking of hæmorrhage, which proceeded from a huge mass of epithelioma projecting into the vagina. There could not have been a more unfavourable case. This specimen is now so shrunken that it does not represent more than a third of the mass which filled up the vagina. When she was admitted into hospital the case was a hopeless one, still I thought it my duty to do something for her. On passing my finger up, I thought I detected tolerably healthy tissue above the growth which sprang from the lips of the os uteri, and I resolved to amputate the cervix—an operation which I have performed before with some success, at least to the extent of prolonging the patient's life a few weeks or months. This operation was difficult, because I could see nothing except the mass, and, indeed, could feel little else, so completely did it fill the vagina. I succeeded, however, in passing a wire beyond what I deemed to be the diseased tissues, and believed I had reached tolerably healthy structures, and in removing this mass without difficulty, but I then became aware that the wire of the ecraseur had drawn in a fold of vaginal wall, and that I had opened the cavity of the peritoneum. Under the circumstances, I thought it best to leave the patient alone, such cases not being necessarily fatal. After some time

she rallied, and I thought she might survive for a time. There was no hæmorrhage, but some occurred subsequently, and she died within forty-eight hours. The exact cause of death was not evident, but I think she died of collapse. There are two lessons to be learned from this case. In writing on the subject of amputation of the cervix uteri, I have pointed out the possibility of the accident occurring, which here happened—namely, opening the peritoneum while using the ecraseur. In this case a very remarkable condition existed, which I never saw before—namely, that the cancerous mass grew upwards as well as downwards. It was, in fact, to a certain extent, mushroom-shaped, and to this condition I attribute the accident. The second inference to be drawn from the present case is this—that Marion Sims' operation is safer than amputation of the cervix. In Dr. Sims's operation we should have scooped all this mass away. I was deterred from that operation by the size of the mass, which was so enormous that I did not think I could scoop it all away. In this, I think I was mistaken, and I am now satisfied that his operation is the safer one of the two, and that it would have been better to have removed all this mass with a scoop, and then dissected upwards. Neither operation, however, would have sufficed to save this patient's life, for, in addition to this large diseased mass which was removed and which you see here, Douglas's space was filled with cancerous matter. If I had not operated, the patient would have died within a fortnight; but if I had not opened into the peritoneum, she might have lived a month or two. While, therefore, I regret the failure of the operation and the unfortunate accident which resulted in the patient's death, still I look on the operation as justifiable; and while I take warning for the failure, I cannot say that I blame myself for the result.

The PRESIDENT.—Had she peritonitis?

DR. ATTHILL.—No; she died from collapse. The opening of the peritoneum is not necessarily fatal. I know two cases of it in which the patient recovered where this occurred.

The Society then adjourned.

MODE OF ADMINISTERING CHLOROFORM INTERNALLY.

POUR the amount of chloroform prescribed into from three to three and a half ounces of pure or sweetened milk, aromatised with a few drops of cherry-laurel water, and shake briskly. The chloroform separates easily into a number of globules, just like the fat globules of milk, in which it remains indefinitely suspended.—*Union Méd.*

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

SESSION 1876-77.

President—RICHARD ROSS, M.D.
Honorary Secretary—WILLIAM WHITLA.

Tuesday, November 28, 1876.

DR. ROSS, President, in the Chair.

Case of Tertiary Syphilis ; with Exfoliation of the whole Anterior Surface of the Axis. By F. E. BECK.

MR. W. came under my care some nine years ago. He was suffering from a chancre, which turned out to be of the true Hunterian character. It left for over three months after it healed the little hard button-like base so characteristic of these sores. He was treated with five-grain doses of the compound subchloride of mercury pill three times daily. The sore was dressed with a mild ointment, composed of the red oxide of mercury. To all appearances the case did well; the sore healed up without any bubo being produced; the induration likewise disappeared. Some twelve months afterwards he called upon me; he was now suffering from well-marked secondary symptoms in the form of a sore throat, spots on his legs, &c. I put him on five-grain does, afterwards increased to eight grains daily, of pil. hydrarg. He was also ordered a mixture containing iodide of potash, iron, and a bitter infusion. Under this treatment he made a rapid recovery. I made him continue the medicine for over five months more or less continuously, until every trace of the syphilis had disappeared.

For a number of years he kept quite well, when he again, for the third time, came under my care. This time he was suffering from a well-marked tertiary spot upon his forehead up among the hair; severe pains about his head, neck, and shoulder; his throat also was sore, and his general appearance was very bad. I ordered him a mixture of iodide of potash, ammon. cit. of iron, and infusion of gentian; told him to take extra nourishing diet, and keep himself warmly clad. In spite of all my efforts, the spot on the forehead enlarged, sloughed, and only healed after I removed a piece of necrosed bone about the size of a half-crown; the piece consisted of the outer portion of the frontal bone. In trying to heal the sore I had some difficulty; what eventually closed it up was lint soaked in a saturated solution of chlorate of potash, with oiled

silk over. After this little operation he came round rapidly. Some two years passed, when he appeared again, for the fourth time, three months ago. This time he complained bitterly of his throat—nothing else. It was impossible to see down the throat, owing to his having a creak or stiffness in his neck; head, neck, and shoulders seemed as if made of one piece. I tried every means that I could think of. When I tried to bend back the head, he bent his whole body backwards, and the pain was very severe; his breath was very offensive. I came to the conclusion that he was suffering from severe ulceration, complicated with some destruction of bone. He was again ordered the iodide of potash and iron mixture—his throat to be well sprayed with Condy's fluid. He reported himself worse and worse for a period of six weeks. I was getting uneasy about what would be the ultimate result of the case. Blood and mucus were coming up very frequently, and the pain was becoming intolerable, when suddenly one night he wakened up, choking, and coughing up mouthfuls of blood. When trying to cough up the blood and get breath, a lump of something came away, all covered with a clot. His wife and he examined it, washed it, stuck their nails into it, and came to the wise conclusion that it was bone, and so it was. On minute examination it proved to be the whole anterior portion of the body of the axis. Since this happened he has gone on well, and is, as far as I or he can know, quite recovered, and at his business. I should add that several pieces of mucous membrane were expelled a few days after the bone came away.

I think this case is peculiar from the fact of the tertiary symptoms being so well-marked and so severe, although he was treated *secundum artem* both for primary and secondary. He was, in the first instance, kept on mercury until the total disappearance of the induration—I always make it a point to do this—and in the next stage, or secondary, he was kept well under treatment until all symptoms had cleared off. That he really was free from syphilis was more or less proved by the fact of his wife having had several children subsequently all healthy, and nursed by herself; moreover, she herself never showed any symptoms of the disease. Lastly, it seems strange how such a portion of bone could come away, and yet the patient make such a good recovery.

The majority of the members present considered the specimen to be part of an upper cervical vertebra.

DR. GRIBBEN, speaking on the treatment of syphilis, said after a long experience he never treated a case of syphilis without opium. He believed, from numerous cases brought before him, that the absence of this drug caused eventually tertiary symptoms to arise, and that in a very large percentage of cases of tertiary syphilis, on inquiry he found there

had been no opium used. He ventured, he said, no explanation, but had so often remarked this he thought it right to mention it.

DR. FAGAN considered the specimen to be the altered body of the axis. He thought that, as regarded the offspring, the period most favourable for transmission was about the time the primary sore healed. ·

DR. J. MOORE referred to the exanthematous theory of syphilis, mentioning the debt the profession owed to Jonathan Hutchinson for his labours in this field. He related some cases of congenital syphilis which came under his notice in the County Prison, and which seemed to get well without any treatment after running the course of an ordinary feverish attack.

The PRESIDENT (DR. ROSS), in thanking Dr. Beck for his paper, agreed with the remarks made, but considered that, from what they knew at present of the nature of syphilis, it would not be justifiable to allow the congenital affection to run its course without treatment.

Case of Retention of Urine depending upon Organic Stricture of Urethra, necessitating Puncture of the Bladder above the Pubes and per Rectum. By J. W. BROWNE, M.D., who exhibited the patient.

JOSEPH BRYANT, aged forty-three, a foundry labourer, residing at Coar's-lane, Belfast, was admitted into the Belfast Royal Hospital, under my care, upon the morning of the 27th May, 1876, suffering from retention of urine. He gave the following history :—He had been a hard liver all his life. Twenty-three years ago he had an attack of gonorrhœa, the acute symptoms in a short time yielding to treatment, but followed by a gleet, from which he has suffered up to the present date. For the cure of the gleetish discharge he has used various injections recommended by sympathising friends.

Six years since he noticed the stream of urine becoming small, and slightly twisted, and after a drunken spree he was seized with retention of urine. A medical man in town catheterised him with difficulty, lacerating the urethra considerably.

From that time till four years since he laboured at various periods—when exposed to cold, or indulged in the use of alcohol—under attacks of retention of urine, which were always relieved by the use of opiates and warm baths.

Some years since he was admitted into hospital, under the care of my colleague, Dr. Murney, suffering from retention of urine. Dr. Murney, after several attempts, succeeded in passing a No. 1 probe-pointed catheter. He remained under treatment thirteen weeks, and was treated by

the method of gradual dilatation. Before his leaving hospital Dr. Murney was able to pass a No. 9 bougie. Three weeks after leaving hospital the stricture commenced to contract, and in a few months the stream of urine was smaller than it had been previously.

Upon the 24th May, three days before his admission under my care, he had been drinking to excess, and was exposed to the cold ; and upon the morning of the 25th May he was seized with retention of urine. Some medical man endeavoured to catheterise him, but failed.

He was admitted into hospital upon the 27th May, at 11 a.m. The house-surgeon gave patient a warm bath and an opiate. At the expiration of an hour, no urine being voided, the house-surgeon and pupil on duty each endeavoured to pass a catheter, but without success, a quantity of blood flowing from the urethra. He was ordered a warm bath, and liq. opii sed. ʒss. every four hours. The opium was continued until 7 grs. had been taken.

At 2 p.m. I visited the patient, and found the bladder distended above the umbilicus. He was suffering great agony; pulse, 120; temp., 101°. I ordered the rectum to be packed with ice, and to take tinct. ferri perchl. mins. xx. every half hour. At 4 p.m., patient suffering great pain, no urine has been voided ; the ice in the rectum had to be discontinued, as it produced great depression. I now tried to pass a catheter, but failed. Dr. Murney met me in consultation at 4 30 p.m., and chloroform being given, he endeavoured to catheterise patient, but failed. The man was suffering greatly, the bladder fully distended; and taking into account the man's previous history, I feared that, as he was probably labouring under organic disease of the kidneys, uræmic symptoms might arise, and owing to the strong spasmodic efforts of the bladder, rupture of that viscus would occur. Dr. Murney agreed with me upon immediate action, and at his suggestion I performed the supra-pubic operation for tapping the bladder, and drew off three and a half pints of highly-coloured urine. I tied the canula in the bladder, and ordered gr. 1 of opium every fourth hour. Beef-tea and milk *ad libitum.*

27th May (evening).—Pulse, 75 ; temp., 100·4° ; patient greatly relieved; has slept some; urine to be drawn off every fourth hour.

28th.—Pulse, 78 ; temp., 100·4° ; patient under the influence of opium ; slight tenderness upon pressure over the right and left iliac regions. Continue treatment, with the addition of linseed meal poultices over the abdomen.

29th.—On arriving at hospital my resident pupil informed me that three hours previously the canula had been removed from the bladder, but by whom I could not ascertain—the patient blaming the nurse, and the nurse stating the patient had removed it. However, at 9 30 a.m., out it was. Upon examination I found a diffuse redness over the region of the bladder. The patient complained of a burning pain in the pelvic

cavity; pulse, 120, and very weak; temp., 101°; evidently urinary extravasation had taken place. Dr. James Moore saw patient in consultation, and he also tried the catheter, but failed. Continue opium and linseed poultices, and have a warm bath. At 3 30 p.m. the patient was very weak, and the diffuse redness over the abdomen had become extensive. I determined to get into the bladder by some means. Three methods presented themselves :—(1). Puncture through the rectum. (2). Opening the membranous portion of the urethra through the perinæum. (3). Endeavour to pass a catheter through the abdominal puncture into the bladder. Taking into account the man's debilitated constitution, and, as I said before, the probability of organic disease of the kidneys, I at once discarded the idea of perinæal section. I first very gently tried to pass a catheter into the bladder through the abdominal wall, but failed in finding the opening in the bladder. I now determined to puncture the bladder through the rectum, which operation I performed in the usual manner with Cook's trocar and canula. Drawing off ten ounces of highly-coloured urine, I tied the canula in the bladder. He was ordered gr. 1 of opium every four hours; linseed meal over the abdomen, and brandy $\mathfrak{Z}$ss. every hour, with milk *ad libitum.* Evening.— Pulse, 134; temp., 102°; diffuse redness over the right flank, and slight pitting upon pressure. Two free incisions were made, giving exit to some fluid possessing a strong urinous odour.

30th.—Pulse, 127; temp., 101°; abdomen tender upon pressure. The diffuse redness has now extended to left iliac region; it is much less upon the right side.

31st.—Pulse, 116; temp., 100°; tenderness all over the abdomen. Some urine flowed to-day per urethram, and a little per rectum, by side of canula; diffuse redness not so extensive, but of a darker hue.

1st June.—Pulse, 110; temp., 102°; complains of less pain upon pressure over the abdomen. Drew off eight ounces of urine through the canula, having a highly alkaline reaction, and containing a large quantity of mucus. Some urine passed per urethram to-day.

2nd.—Pulse, 100; temp., 100·2°; tongue furred; tenderness over abdomen increased; complains of pain over region of bladder, and along the dorsum of the penis, which is inflamed. The canula was withdrawn to-day.

3rd.—Pulse, 100; temp., 101°; passing urine, one tablespoonful at a time, every half hour; pain and redness around the abdominal puncture; matter is evidently forming.

4th.—Pulse, 98.; temp., 100°; pus flowing freely from the abdominal puncture; urine comes more freely per urethram. Ordered the wound in the abdomen to be syringed with Condy's fluid. Bowels constipated; ordered a stimulating enema.

5th.—Pulse, 100; temp., 98°; had my attention drawn to-day to an

abscess which had been slowly forming over the region of the right pectoral muscle; urine coming pretty freely per urethram; patient's expression greatly improved.

I need not detain the members by a detailed account of his daily progress. From June 5th he suffered from constipation of the bowels, which was relieved by enemata.

Upon the 13th he was seized with diarrhœa, frequently passing ten and twelve fluid stools in the twenty-four hours. Nearly every remedy in the Pharmacopœa was tried to check the diarrhœa, which eventually yielded to a mixture containing acetate of lead, acetic acid, liq. morphia, and tannic acid. He complained frequently of tenesmus of the lower part of the rectum, and a peculiar burning pain. This was relieved by an enema of liq. bismuthi and starch. His pulse varied greatly during his illness, as also did the temperature. The general average of the pulse was 115, and the temp. 101°.

The abscess over the pectoral muscle, upon the 19th June, was found extending downwards; in a few days it got into the sheath of the rectus abdominis, and eventually discharged itself at the umbilicus.

Upon the 24th June I succeeded in passing a No. 4 gum-elastic catheter into the bladder, which I tied in and retained. In the course of twenty days I was able to pass a No. 11 bougie. All urine had now ceased to flow per rectum and through the abdominal puncture.

The patient during his long illness was, fortunately, able to take a fair amount of nourishment, which consisted of milk, eggs, and brandy. Upon the 7th August he was discharged from hospital, passing urine in a good stream, and feeling wonderfully strong.

I saw this patient upon the 8th November, 1876; he was then able to get about the house, and walk a distance of half a mile. The umbilicus looked red and inflamed; the wound in the abdomen had quite healed; no urine coming either through the abdominal puncture or per rectum.

The members present congratulated Dr. Browne upon the success of his case.

INJECTION OF PULMONARY CAVITIES.

In a Report from the St. Francis Hospital, published in *The N. Y. Med. Rec.* (Nov. 11, 1876), it is stated that in several cases a hypodermic needle had been thrust through the chest-walls into cavities in the lungs, and a few drops of a one and one-half or two per cent. solution of carbolic acid injected. No bad effects were produced, and the operation seemed to lessen the expectoration and cough, and thus to have modified the fever. [Further particulars as to the local treatment of cavities in the lungs will be found in *The Irish Hospital Gazette*, Vol. II., pp. 140 and 868; and Vol. III., p. 44.—ED.]

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

President—THOMAS HAYDEN, F.K.Q.C.P.
Secretary—E. H. BENNETT, M.D.

Intra-thoracic Aneurism.—DR. HAYDEN said: I beg to submit to the Society an example of intra-thoracic aneurism, which exhibits an unusual course and termination. A man aged twenty-eight, a painter and decorator by trade, and most eminent in his calling, but unfortunately intemperate, called at my house on the morning of the 14th of last September. I had previously known him, and, in fact, engaged his services as a painter. He complained of being very ill, but still was proceeding to his work, as indicated by the brush which he carried in his hand. A very brief examination, made while he stood in my hall, satisfied me that he was the subject of aneurism, and in a most critical condition. I advised him to go to hospital at once, and gave him a ticket of admission. I visited him in hospital in the course of that morning, and learned that he had felt quite well up to eight weeks before his death. He then began to suffer from pain between his shoulders. He also had what he described as a "stuffing" in the upper part of his chest; and could with difficulty swallow solids, and was obliged to take a mouthful of liquid to force them down. The right radial pulse was 84, full and regular, while the left was hardly to be felt. The right carotid artery pulsated strongly, the left very feebly. There was loud laryngeal stridor, which attended both inspiration and expiration, and was distinctly audible along the lower cervical and upper dorsal spine. There was also a "brassy" cough. He complained of pain between his shoulders, but had then no difficulty in swallowing. The heart pulsated in the normal position, and with the ordinary force and regularity. The sounds in the præcordial region were normal, but I was struck at once with the increasing loudness of both sounds of the heart as I traced the aorta upwards in the course of the arch. The sounds attained a maximum intensity over the left half of the transverse portion of the arch, and were of a sharp ringing character. Interposed between the first and second sounds of the heart, following the first, and as it were appended to it, was a blowing murmur, both sounds being distinctly audible. The left pupil was contracted to half the size of the right. There was no engorgement on the left side of the neck. The diagnosis was exceedingly simple. An aneurism obviously engaged the left portion of the arch of the aorta, involving or pressing on the right subclavian artery, and, most probably, also the trachea, and the left recurrent and

sympathetic nerves. I may observe, as to treatment, that by repeated leeching and large doses (20 grs. thrice daily) of iodide of potassium, the man was entirely relieved of all his urgent symptoms. The pain subsided; he could lie down and sleep; was enabled to take his food, and partially recovered his voice. In this condition he remained up to the 21st of September. On the 20th he was so well that on my going into the ward he presented me with a sketch he had intended for my dining-room, beautifully executed by him on the previous day. He said he felt quite well, and besought me to allow him to get up, but I did not think it discreet to concede that favour. He therefore remained in bed. On the following day he was quite well up to seven o'clock in the evening, when he suddenly became faint, and died in about three minutes, while sitting up in bed. The viscera are now before you. My clinical assistant, Mr. Browne, on opening the abdomen found in the peritoneal cavity several pounds of blood partly coagulated and partly liquid. To the left of the œsophageal opening in the diaphragm there was a rent in the peritoneum through which my thumb passed. Around this aperture the blood was infiltrated to the extent of an inch. The aperture led into a channel as large as the œsophagus, and running upwards, parallel with it, but on the left side. It was lined with laminated fibrin, disposed in several layers, and terminated above the left curvature of the arch, in an aneurism as large as an orange. It pressed upon the antero-left aspect of the trachea near the bifurcation, and also on the left bronchus, and, as you see, communicates with the aorta by an opening as large as the end of my thumb. It is lined with laminated fibrin, the section of which shows that it is remarkably thick at all points save one—namely, the left anterior portion of the sac; here it is thin, and here exists the rent by means of which the aneurism had communicated with and formed a channel in the posterior mediastinum, leading to the rent in the diaphragm. Manifestly the aneurism had long been of the secondary false variety, as judged by the firm and laminated condition of the fibrin which lined the channel in the posterior mediastinum. The left subclavian artery was pressed upon at its origin, and nearly occluded for a distance of an inch by the aneurism. Hence the failure of the left radial pulse. The sac also had pressed on the trachea and left bronchus, whilst the recurrent nerve, where it passed round the aorta, was incorporated with the sac. Hence the laryngeal symptoms. The aorta is atheromatous, and on the right side scabrous; the heart is perfectly sound. The left ventricle is normal, but in the state known as that of concentric hypertrophy. This was due to the suddenness of the man's death. There is no cardiac disease whatever. The case is interesting—first, because of the course taken by the escaped blood. The immediate cause of death seems to have been the rupture of the diaphragm, and the pouring of blood into the peritoneum. I wish to direct particular attention to the

murmur which was localised in the left side, and which I would designate as post-systolic in rhythm. A murmur of this precise rhythm I had not previously heard in an artery. It is not uncommon at the mitral orifice, as I have elsewhere shown. I have long held that the sounds heard in aneurisms of the arch are transmitted cardiac sounds. If we admit this—as I think we must—the difference in the time occupied in the transmission of a blood-wave and a sound-wave from the heart to the aneurism, assuming both to have been so transmitted, would explain the want of coincidence between the first sound and the murmur in the aneurism, the former being transmitted and the latter auto-chthonous.—*December* 2, 1876.

Syphilitic Necrosis of the Skull.—MR. F. T. PORTER said: This is a specimen which a fortnight ago was removed from the body of a middle-aged male subject in the Ledwich School of Medicine. The history of the subject is very obscure, but I ascertained through the kindness of Mr. Kenagh, a student of St. Vincent's Hospital, that four or five months ago the man was an out-patient of that hospital, and that he subsequently went to the South Dublin Union Hospital, where he died, and that there were no very marked symptoms. The specimen is compara-tively rare. Part of the inner table remains at the upper margins of both parietal bones, extending two inches along the sagittal suture, and at the centre and right half of the frontal bone the whole substance of the bone is perforated and completely destroyed. There are two matters of interest connected with the specimen. One is that, although the man admitted to Mr. Kenagh that he had contracted syphilis six years previously, yet the most diligent examination that we could make of his penis, and urethra, and groin, did not detect the slightest trace of a *primary sore* of any kind, or the remains of any *bubo*. The anterior surface of each leg, however, about the centre of the tibia was occupied by a cicatrix about two inches square; and there were patches of syphilitic psoriasis over different parts of the body. The tongue was in a state of chronic glossitis, and the teeth were in a condition such as might be produced by the prolonged use of mercury. In the right Whartonian duct was a ranula of considerable size. The liver and spleen were both enlarged. The liver passed considerably below the umbilicus, and was not only enlarged but altered in its consistency, being almost cartilaginous in hardness, as was also the spleen. Another interesting point is that there appears to be no affection of the bones of the nose, whereas in nearly all tertiary syphilitic disease those bones are generally more or less affected. Knowing nothing of the history of the case beyond what Mr. Kenagh stated, I cannot very well account for the appearances, but I think the disease must have commenced in nodes and proceeded to subsequent exfoliation.—*December* 2, 1876.

Mediastinal Lympho-sarcoma.—DR. WALTER SMITH said: The case I
desire to show to the Society is one of a primary mediastinal new
growth, a disease which certainly must be reckoned one of the uncommon
diseases, and it is that variety of mediastinal tumour which makes up
the bulk of the cases clinically grouped as "intra-thoracic cancer." A
gate-keeper, aged forty-five years, was admitted into the Adelaide
Hospital on the 1st of September, 1876. His family history was re-
markably good. His father had died at the age of seventy-six,
apparently of simple senility, and his mother was alive and aged eighty.
The man himself was robust, and of large build, and had enjoyed
excellent health up to the beginning of last April. At that time he
was exposed more than usual to the weather, and contracted a severe
cold, without sore throat, but which was followed by a hoarseness and
a choking sensation in his chest, as if there was there a quantity of
phlegm which he could not get up. He was also affected with pain
from his left breast to the corresponding ear, which was relieved by
external applications of iodine. About the middle of May spitting of
blood set in, and he coughed up a number of dark clots. This hæmo-
ptysis continued until the second week in August, when it ceased. About
the middle of June he was again attacked with pain in the left mammary
region, which disappeared, but returned for a time, and then again dis-
appeared. By this time he was so weak that he was obliged to give up
his occupation. At the end of July a large nodulated tumour appeared
above the left clavicle, while that arm became painful and œdematous,
and he lost his voice completely. He had also paroxysmal fits of
coughing, which were attended with shooting pains. His condition when
admitted to hospital was as follows:—The expression of his face was
anxious and distressed. He reclined in a semi-upright position, and
was tormented by urgent dyspnœa. The right side of his chest was
resonant anteriorly and posteriorly, and in short normal in every respect,
except for a small dull space to the right of the middle line of the sternum.
Down the rest of the middle line of the sternum there was an abrupt line
of dulness, which extended completely over the left side back and front,
and percussion was attended with a notable sense of resistance. The
left intercostal spaces were widened, and there was œdema on the left
side of the chest, which was enlarged. Some of the cutaneous veins were
dilated, and the left axillary glands were swollen. On auscultation
distant tubular breathing was detected, but no moist râles of any
kind. Stridor "from below" was distinctly marked. The heart was
weak, and its apex displaced downwards towards the right side, but
there was no murmur either at its base or apex. The sputum was very
tenacious, buff-coloured and tinged with blood in intimate mixture with
it. The urine presented characters which have been remarked in other
cases of malignant visceral disease, being of a clear brown colour, and

darkened considerably in the cold by nitric acid. When mixed with an equal volume of hydrochloric acid no change was produced until it was heated, when it became of a dark brown colour, similar to that produced by Moore's test for sugar, depositing a granular brown sediment which seemed to indicate the presence of indican in the urine. The left pupil was contracted under the influence of either a bright or a medium light, and was always smaller than the right. The left fore-arm was larger by an inch and a half than the right, and was doughy and œdematous to the extremities of the fingers. The left radial pulse could scarcely be felt, and sensation was deficient along the inner side as far as the wrist. He gradually failed throughout September; on the 16th of that month he was nearly choked with phlegm, and became suddenly pallid, but rallied for a short time, and lingered in a state of great distress, with intense dyspnœa, until the morning of the 18th of September, when he died. A *post mortem* examination, made nine hours after death, revealed the following appearances:—The chest and abdomen were examined, but the head was not opened. On lifting up the skin of the chest a hard nodulated gland was found in the second left intercostal space superficially; but with that exception no external tumour of any kind was discoverable. Previous to the autopsy a puncture made in the left side of the chest caused a small amount of a reddish-brown fluid to escape. The left thorax contained a large quantity of a reddish-brown fluid in the lower part of it, and also a large number of soft gelatinous red clots, some of which also existed on the right side. The left pleura was greatly thickened and areolated on its surface with small depressions. The inner surface of the pleura was deeply pigmented. The right lung was normal, with the exception of a slight amount of congestion, and a few fleecy adhesions at the upper part. The whole of the mediastinal partition was blocked up by an enormous wedge-shaped growth projecting anteriorly at the right of the sternum, and adherent to it—thus accounting for the limited dulness observed during life. The mass was continuous with the supra-clavicular tumour which was palpable during the patient's life. The new growth ran down the back of the œsophagus, which was adherent along the whole extent of it, and the gullet was considerably narrowed at the upper part. The patient had complained of dysphagia. The glottis was œdematous, and covered with viscid muco-pus. One side of the left subclavian artery was indented by the pressure of the new growth; and it is of some interest to observe that the new growth merely pushed in the walls of the vessel. On examination by the microscope no evidence was found of the disease having invaded the interior of the vessel. The mediastinal mass was solid throughout, and on fresh section presented a yellow-white creamy appearance. There was no essential difference shown on a section of any part of the neoplastic mass. The left lung was reduced to a thin,

brown, soft, friable layer, attached to the side of the mediastinal tumour, and the pleura enveloping it was thick and leathery. The pericardium presented a remarkable appearance. It was distended with a large amount of hæmorrhagic effusion, somewhat resembling tincture of kino. In the back of the pericardium, on turning up the heart, there was a semi-circular zone of nodules, which, in the recent state, were festooned with a bright red arborescent line of vessels. At the roots of the great vessels, especially around the pulmonary artery, were a number of hard tuberous nodules. The interior of the heart was healthy, and there was nothing wrong with the valves. In the abdomen there was nothing calling for remark beyond the fact that a few of the mesenteric glands were enlarged. Some of the pericardial fluid was examined under the microscope, and there were observed in it large brown cells with clear borders and large granular nuclei. A drop of the creamy juice from the cervical tumour showed a number of small granular cells of nearly uniform size, some having undergone fatty degeneration. Fresh sections were examined with the microscope, and other specimens were hardened in Müller's fluid. Dr. Purser also examined the fresh specimens, and determined the structure of the tumour to be that of lympho-sarcoma. Thin secretions showed abundant, rather broad fibrous trabeculæ, the inter-spaces blocked up with multitudes of large lymphoid cells. The diagnosis, being that of a malignant tumour pressing on the lung, rather than that of a malignant growth infiltrating the lung, was arrived at without difficulty during life, to the exclusion of pneumonia, simple pleurisy, phthisis, aneurism, or mediastinal abscess. It was inferred during life that it was lymphomatous, from the hard mass of cervical glands, coupled with the absence of infiltration in other neighbouring parts and the rapidly malignant course of the disease. The large amount of pleural hæmorrhage and pericardial effusion coinciding with what has been observed by other physicians, is noteworthy, it being known that in doubtful cases the evidence of bloody effusion on tapping has led to the diagnosis of cancerous pleurisy. The condition of the parts showed what would have been the inutility of tapping the chest during life. The total duration of this case was only five months. The shortest case on record is one of Jaccoud's, in which the total duration was only eight days from the first development of the subjective symptoms. The specimen has been preserved since the 18th of September in a solution of chloral, consisting of five grains to the ounce of water, and is now sweet and free from any evidence of decomposition. The solution was changed once.—*December* 2, 1876.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, December 30, 1876.

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhœa	
Dublin, -	314,666	682	593	4	17	11	—	6	39	10	24·5
Belfast, -	182,082	474	340	—	6	9	1	17	8	12	24·3
Cork, -	91,965	158	177	—	—	—	1	—	3	4	25·0
Limerick, -	44,209	68	81	—	—	1	—	1	2	4	24·0
Derry, -	30,884	64	29	—	—	—	—	—	—	—	12·3
Waterford, -	30,626	80	61	—	—	1	—	—	5	1	26·0
Galway, -	19,692	37	26	—	—	1	—	—	1	1	17·0
Sligo, -	17,285	32	21	—	—	—	—	—	—	2	15·8

Remarks.

Waterford, Cork, Dublin, Belfast, and Limerick had a slightly high death-rate in the period. The other towns showed a very low rate. In London the death-rate was 21·9 per 1,000 of the population annually, in Edinburgh 22·5, and in Glasgow 23·9. The corrected rate in Dublin was 23·7. Zymotic affections caused 114 deaths in Dublin, against 120 in the previous four-week period. Fever and measles showed a considerable increase, while scarlatina declined, and small-pox was not more fatal than before. Whooping-cough continued as an epidemic in Belfast, and fever was fatal in Waterford. Of the 39 fever-deaths registered in Dublin, 7 were from typhus, 23 from typhoid, and 8 from simple continued fever. In London small-pox caused 297 deaths, compared with 210 and 74 in the two preceding periods of four weeks each. The weekly numbers of deaths were 50, 75, 97, and 75 respectively. The total deaths from small-pox in London during the 52 weeks ending December 30, 1876, were 735.

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METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20' N., Long. 6° 15' W.,
for the Month of December, 1876.

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Mean Height of Barometer, - - -	29·350 inches.
Maximal Height of Barometer (on 9th at 9 p.m.), -	30·169 „
Minimal Height of Barometer (on 4th at 7·15 a.m.),	28·448 „
Mean Dry-bulb Temperature, - - -	44·5°
Mean Wet-bulb Temperature, - - -	42·9°
Mean Dew-point Temperature, - - -	41·0°
Mean Elastic Force (Tension) of Aqueous Vapour,	·257 inch.
Mean Humidity, - - - -	87·5 per cent.
Highest Temperature in Shade (on 27th), -	55·2°
Lowest Temperature in Shade (on 24th), -	30·3°
Lowest Temperature on Grass (Radiation) (on 24th),	26·2°
Mean Amount of Cloud, - - -	71·0 per cent.
Rainfall (on 22 days), - - - -	7·566 inches.
General Direction of Wind, - - -	S.E. and S.W.

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Remarks.

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This month was remarkable for an unusually low mean barometer, a persistence of southerly and south-easterly winds, a high mean temperature, and an excessive rainfall. Very deep depressions were observed, and, owing to extreme cold and a high barometer in Scandinavia, their progress eastwards was arrested, so that they generally travelled in a curve from S.W. to N. across the British Islands. The most serious depressions were those of the 2nd to 6th, and of the 19th to 22nd. In the former the barometer in Dublin remained below 29 inches for some 96 hours! In the latter, owing to high pressure in Scandinavia, barometrical gradients for E. winds became very steep in Scotland, and a terrible gale, with severe snowstorms, prevailed for many days before Christmas. In Dublin the only cold weather of the month occurred at the same time. The rainfall exceeded that recorded in the memorable October of 1875 (7·059 inches), and was by far the greatest registered in any month during the past twelve years. Lightning was seen on the 17th and 20th. Hail fell on the 5th, 21st, 25th, and 26th. A little snow fell on the morning of the 22nd. On the 2nd and 3rd no less than 1·828 inches of rain were registered in twenty-four hours.

Rainfall in 1876,

At 40, *Fitzwilliam-square, West, Dublin.*

Month	Total Depth	Greatest Fall in 24 Hours		Number of Days on which ·01 or more fell
	Inches	Depth	Date	
January, - - -	·406	·119	19th	9
February, - - -	3·012	·489	13th	23
March, - - -	2·158	·400	5th	23
April, - - -	2·601	·469	9th	17
May, - - -	·798	·305	22nd	6
June, - - -	1·269	·436	13th	14
July, - - -	1·337	·679	26th	10
August, - - -	2·260	·910	2nd	14
September, - - -	3·146	·464	3rd	17
October, - - -	4·505	·954	10th	20
November, - - -	3·614	·538	16th	20
December, - - -	7·566	1·828	2nd	22
Total, - -	32·663	—	—	195

GLYCOSURIA IN THE PUERPERAL STATE.

M. GUBLER has communicated his researches to the Société de Biologie. He finds that when the abstraction of milk is suspended through the death of the infant, or when it is suppressed through a slight illness of the mother, glycosuria is the rule. In a serious affection, such as enteric fever, sugar is never found in the urine. If purgatives be given when the milk is suppressed from a slight illness, and the intestinal canal affected, glycosuria does not result. The quantity of sugar is never very considerable; but it will reveal itself to all the usual reagents, that of Luton (solution of bichromate of potass and sulphuric acid) giving the most abundant precipitate. The glycosuria appears at the end of 24 hours after the flow of milk is stopped, and may last a week. Sometimes it is found in the case of pregnant women, but these are generally primiparæ, towards the close of the period of gestation.—*Le Progrès Médical.*

S. W.

PERISCOPE.

Edited by G. F. Duffey, M.D., F.K.Q.C.P.

TEST FOR ALBUMEN.

MM. Bouchard and Cadier (Société de Biologie, Oct. 21, 1876), note several causes of error in the test for albumen according to the method pointed out by M. Tauret, in 1873, and give the means of avoiding these causes of error. The test is the double iodide of mercury and of potassium acidified by acetic acid. It is prepared by dissolving in distilled water some per-chloride of mercury by means of iodide of potassium, which is added gradually, with agitation, until all the red iodide of mercury, first formed, is re-dissolved. MM. Bouchard and Cadier also recommend the addition of an excess of iodide of potassium, and, finally, a large proportion of acetic acid. The causes of error are five in number:—1. If iodide of potassium has not been used in excess, there may be, in the normal urine, a precipitate of iodide of mercury, which may be confounded, when very scanty and viewed by transmitted light, with the white pre-cipitate of albumen. 2. Mucine is also precipitated by the acetic acid of the test, but it has its peculiar transparency, and its deposit is globular. It is easy to avoid this error. 3. The urates, if abundant, in neutral or slightly acid urine at a low temperature, may be precipitated in the form of acid urates by the acetic acid of the test; this precipitate disappears on heating the tube, whilst the albuminous precipitate becomes more marked. 4. In alkaline urine, if a very small quantity of the test is added, the mixture remains acid, and there is a white precipitate of the double iodide of urea and of mercury; but this precipitate passes almost immediately from a deep grey to black, which does not take place with the albuminous precipitate. 5. The alkaloids form a precipitate with this test, but it disappears by applying heat or by treating with alcohol. In spite of these causes of error, the test, which is a most delicate one, deserves a place in the clinique; since having noticed the causes which are likely to lead to mistakes in its employment, it is easy to avoid making them.—*Gaz. Hebdom.*, October 27.

ON THE CURRENT OF THE FLUIDS IN THE LIVING EYE, AND IN THE TISSUES IN GENERAL.

In *Virchow's Archiv.*, Vol. LXV., p. 401, is an article "Zur Lehre von den Flüssigkeitsströmungen im lebenden Auge und in den Geweben überhaupt," by Dr. Max Knies. It is the result of experiments made by him in Prof. Kühne's physiological laboratory at Heidelberg. The

experiments consisted in the injection of one or two drops of a 10 per cent. solution of cyanide of potassium into the vitreous humour of living rabbits. After the lapse of from one to four hours the animals were killed, and the eyes hardened before being examined. The following are the conclusions the author arrives at after the examination of a number of eyes treated in this way:—" 1. There exists in the interior of the eye a general current of the fluids from behind forwards. 2. The existence of this current even in the crystalline lens admits of proof. 3. The nutrition fluid for the lens first passes through the vitreous humour, and thus corresponds in the direction of its stream to that in the arteria hyaloidea of the fœtus. The conductor of this nutrition fluid is the intercellular substance of the vitreous humour. 4. The nutrition of the cornea takes place from the anterior chamber by aid of the cement-substance, and intercellular substance of the cornea itself. 5. The aqueous humour is a mixture of transudation from the ciliary body with fluid which has first passed through every part of the interior of the eye. 6. The aqueous humour is discharged from the eye by two ways ; first, through the cornea towards the sub-conjunctival tissue; secondly, from the canal of Schlemm out through the substance of the sclerotic. 7. Paracentesis of the anterior chamber gives rise to a hastening of the nutritive changes in the interior of the eye, and especially in the lens. 8. The intercellular substance is to be regarded as the conductor of the nutritive fluids to the parenchymatous and connective tissue cells in general." In Prof. Theodor Leber's article, "Studien über den Flüssig-keitswechsel im Auge" (*Archiv. f. Ophthal.*, Vol. XIX., pt. 2, pp. 87–185), which received the Graefe medal in March last, he almost arrives at a nearly similar conclusion with regard to a way of exit of the aqueous humour from the eye. He is inclined, namely, to regard the ligamentum pectinatum, and its immediate neighbourhood, as the point at which this fluid chiefly escapes. It filtrates into the canal of Schlemm, and passes off by the anterior ciliary veins and vortex veins. So long as the membrane of Descemet (posterior epithelial layer) of the cornea is intact, the cornea is impervious to the aqueous humour. Contrary views are founded upon observations made on eyes in which *post mortem* changes had taken place in the membrane of Descemet.

H. R. S.

ON GLAUCOMA.

In *v. Graefe's Archiv.*, Vol. XXII., part 3, p. 163, Dr. Max Knies has an article, "Ueber das Glaucom," in which he endeavours, by means of pathological investigations, to decide the cause of glaucoma ; a question which has long occupied the attention of physiologists and ophthal-mologists. He examined fifteen eyes which had been the subjects of various forms of glaucoma. The pathological condition most constantly

present in these eyes was the obliteration of the canal of Schlemm, in consequence of adhesion of the peripherical zone of the iris to the cornea. This had been already observed by Heinrich Müller and others, but it has hitherto been looked upon as merely the effect of the abnormal intraocular pressure. That this is not the correct view Dr. Max Knies considers proved by the observation made by him, that there exists in a more or less marked degree an inflammatory exudation between the iris and the cornea, where they are adherent. The inflammatory process extends to the whole of the immediate neighbourhood of the canal of Schlemm. The author believes that this place is peculiarly exposed to such a process by the fact of its being the point, as shown by him in his article above noted, at which nearly all the fluids of the interior of the eye, especially the vitreous humour, pass out. This fact also makes it evident that obliteration of the canal of Schlemm must increase the intraocular tension. Not only is the cardinal symptom of glaucoma, the increased pressure within the eyeball, satisfactorily accounted for by the existence of an adhesive inflammation about the canal of Schlemm, but also other symptoms, which have hitherto been regarded as the consequence of the pressure alone, will be much more reasonably explained if their origin be sought for in the local alteration referred to. These are—the corneal opacities and anæsthesia, the iridoplegia, the apparent shallowness of the anterior chamber, the paralysis of accommodation, and the hyperæmia of the anterior scleral veins. Although Dr. Knies in the course of his investigations has formed the opinion that the inflammation in the neighbourhood of the canal of Schlemm is a primary one, and not simply the result of increased pressure in the vitreous humour, still he admits that he has not yet succeeded in obtaining objective proof of this view. In most of the eyes which the author examined no iridectomy has been performed, and in only one of them had it been performed successfully. In this one the periphery of the iris continued adherent at the place where the operation had been done. This would speak for the view of Quaglino, Wecker, and others, that the iridectomy, *per se,* plays no part in the cure, but that it is the "filtration cicatrix," situated in the corneo-sclerotic margin, which is alone essential.

H. B. S.

IODINE IN TYPHOID FEVER.

In the St. Francis Hospital (*N. Y. Med. Rec.,* Nov. 11, 1876), iodine has usurped the place of the mineral acids, and is given internally in all cases of typhoid fever. It is thought to lessen the troublesome gastric irritability so often present, and to diminish the number of evacuations from the bowels. The following formula is usually employed:—℞. Iodinii, ʒ j ; potassii iodidi, ʒ ij ; aq. dest., ℥ x. M. Gtts. iij. in wineglassful of water every three hours. Lugol's solution, the liquor iodinii comp. of

the (U. S.) Pharmacopœia, may be used in doses of six drops every three hours. The cold bath, quinine and iodine, with close attention to the diet and the exhibition of stimulants when indicated, constitute the main features in the treatment of typhoid fever. The cold wet pack is occasionally substituted for the cold bath when the latter is not well borne.

CARBOLISED BRAN IN COMPOUND FRACTURES.

DR. LEWIS D. MASON, Brooklyn, New York, says that the advantages possessed by bran, properly mixed with carbolic acid, as a dressing, are these:—1. The discharge is disinfected as it flows from the wound into the bran; 2. We have a dressing that is "germ proof," and one that notably limits suppuration; 3. We secure the anæsthetic properties of the acid. The method of carbolising the bran is very easy—simply by adding the crude carbolic acid slowly to the quantity of bran to be carbolised, stirring it at the same time. A little experience will decide how much of the acid a given quantity of the bran will require. An excess should be avoided. The bran will retain the properties of the acid for some time.—*N. Y. Med. Jour.*, September, 1876.

NEW PREPARATIONS AND SCIENTIFIC INVENTIONS IN RELATION TO MEDICINE, SURGERY, AND HYGIENE.

The Patent Pocket Inhaler. By J. B. AUSTIN.

FOR months back the readers of the advertisement sheets in the medical journals have puzzled over a polysyllabic word, which, by itself, constituted an advertisement. That word was "ALEXIMORHYGIASTIKON." We have been informed that this "death-repelling, health-restoring" word was the name given to a patent Pocket Inhaler, introduced by Mr. James Battle Austin, of London. One of the bottle-forms of this ingenious inhaler has been submitted to us, and we have no hesitation in speaking of it in terms of praise. The inhalant, contained in a neat little bottle, is dropped into the inhaler by means of a wire-dropper, which measures the dose with accuracy. The inhaler, itself, is constructed of bibulous paper, which is pure vegetable fibre, a ready absorbent of liquids, and unlikely to be affected by chemical reagents of all organic substances.

The superior advantages claimed for the inhaler are:—

1. Its cheapness, simplicity, and efficiency.

2. It admits the use of all volatile remedial fluids, in a concentrated form, without dilution.

3. It has a certain and unchanging area of exhaling surface.

4. A definite supply of air, which is in no danger of being varied.

5. It insures nearly an equal rate of evaporation during inhalation.

6. It may be carried in the pocket, and used at pleasure at all times and under all circumstances.

The tubelette on which the inhalant is dropped will, of course, serve only for one inhalant, whether it be chloroform, carbolate of iodine, or so on. But, whenever a physician prescribes a different form of inhalant, extra tubelettes may be obtained of the persons who supply the inhaler.

The prices of the "Patent Pocket Inhalers" are moderate, and we anticipate for them general acceptance at the hands of the profession and of the public.

Sawdust Pad for Necrosis, &c. By Surgeon-Major J. H. Porter, of the Royal Victoria Hospital, Netley.

This is a pad, composed of sawdust derived from one of the pine species, which Surgeon-Major Porter has lately been using as a dressing in cases where there is a copious discharge of offensive pus, such as necrosis and psoas abscess. It acts also as a pad for supporting a stump. He has found it agreeable to the patient—clean, soft, a good absorbent, and a powerful deodoriser, from the large amount of terebene it contains. It is *very inexpensive.*

It is applied in carbolised gauze bags (other gauze will answer), any pieces of hard wood or splinters which may possibly have found their way in during the process of sawing having been first removed. A portion of oiled lint with several holes in it may, if considered necessary, be applied between the pad and the surface with which it is brought in contact.

The idea of using it first struck Surgeon-Major Porter from seeing the absorbing power of sawdust when used at operations, and from observing how fruit—especially grapes—may be preserved when packed in it.

Sawdust derived from hard wood will not answer, as it does not absorb freely. Memel pine contains most terebene, and is therefore decidedly the best.

This dressing will also answer for extemporised pads for fractures, and is likely to prove a valuable addition to the *armentarium* of the practical surgeon.

Surgeon-Major Porter's suggestion is also interesting when considered in connexion with Dr. Lewis Mason's dressing of carbolised bran, described in the "Periscope" (page 207).

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ART. VIII.—*Aphasia as an Effect of Brain-disease.** By CHARLES
ÉDOUARD BROWN-SÉQUARD, M.D., Paris ; F.R.C.P., London;
F.R.S.

APHASIA.

MR. PRESIDENT AND GENTLEMEN,—I have to-day to speak of
Aphasia. You know that we now call by that name the loss of the
faculty of expressing ideas by speech.

Aphasia, since my friend Professor Paul Broca has tried to show
what is the seat of the lesion which produces it, has been considered
as, perhaps, giving one of the best proofs that in the brain there are
distinct centres possessing definite functions. When Professor
Broca first studied aphasia he considered that it depended on a disease
either of the left third frontal convolution alone, or of that convolu-
tion and its neighbourhood. Later on, he thought the faculty lost
in aphasia had its seat only in the back part of the left third
frontal convolution. But other physicians, especially Professor
Sanders of Edinburgh, and Meynert of Vienna, grounding their
views on a number of autopsies, maintained that the seat of the
faculty of expressing ideas by speech was in the left island of Reil.
That part, as you well know, is not very far from the third frontal
convolution; and some eclectic physiologists have looked on both

* A Discourse delivered before the President and Fellows of the King and Queen's
College of Physicians, Monday, November 27, 1876.

the insula and that convolution as being the seat of the faculty of expressing ideas by speech.

I shall, in a moment, come again to the facts which seem to establish that one or other of those two parts is the centre of the power of speech, but before I do so it is well to examine what facts show as regards that mental faculty. There are very interesting cases, indeed, showing that when disease exists in those parts in children before they have learned to speak, or when they have not yet lost the power that we have before the age of six or seven, and sometimes later on, of learning a language with ease, aphasia may not occur. If the left side of the brain be diseased in those spots which are considered as centres of speech, the faculty of speech can, nevertheless, develop itself, if it have not already appeared; and, if it have appeared, it will, nevertheless, after having been altered for the time, come to be just as if nothing had taken place. There are a number of cases of atrophy of the left side of the brain having begun in childhood, and in which, nevertheless, the faculty of speech existed perfectly.

Professor Charcot, and all those who believe in localisation, consider that fact as being not in the least antagonistic to the view held by Broca, and they explain it in this way:—They say that if in childhood the left side of the brain be deficient, the right side can develop enough of the dormant activity that exists in it in all of us to admit of the faculty of expressing ideas by speech becoming perfect.

There are certainly some very interesting facts bearing on this point. If a left-handed man loses the faculty of speech there is a great likelihood that he labours under an alteration of the right side of the brain in those places or in the neighbourhood of those places which have been considered as the centres of speech. I have myself seen two such cases, and there are certainly more than ten or twelve on record to my knowledge, and probably more that I do not know of. So that there seems to be abundant proof, both as regards cases of disease of the left side of the brain in right-handed people, and cases of disease of the right side of the brain in left-handed people, which goes to show that those two parts—*i.e.*, the island of Reil and the third frontal convolution—are endowed with the faculty which has been attributed to them. This would be very well if we were to be content with only examining facts which seem to be in favour of the theory; but, unfortunately, I must say there is a great number of other facts which, notwithstanding the

efforts made to explain them away, are quite antagonistic to the views I have mentioned.

Let us bring face to face first the facts that seem to prove that the island of Reil is the seat of the faculty of speech, and those according to which the third frontal convolution is the seat of that faculty. If you study the cases which have been brought forward by Meynert, and by an able Edinburgh physician, Professor Sanders, and also a good many other observers, which seem to establish that the island of Reil is the seat of the faculty of speech, you will find that in a number of those cases, although not in all, the third frontal convolution was not diseased at all.

Such cases, especially the last ones, seem clearly to establish the correctness of Sanders' and Meynert's view; but, on the other hand, if you take the cases that Professor Broca and his supporters have brought forward, in which the disease was altogether limited to the whole of the third frontal convolution, or only to its back part, you would find that in those cases, although the island of Reil was healthy, the faculty of speech was altogether lost. We may have several opinions about this. We may make the supposition that the two parts are centres for the expression of ideas by speech. But if so, how is it that with an organ which is complex or extensive, or, still more, double—how is it that when only part, only one-half, of that double organ, or one of the two organs if you like, is destroyed, while the other still exists—speech is absolutely lost? If the two are what it is said they are, upon the destruction of only one of them, while the other is left in a healthy state, speech should be only diminished but not lost; whereas in those cases it was absolutely lost.

Now, if you admit another view—namely, that of those who maintain that the island of Reil is the only centre, what do you find if you put face to face with that view the cases in which the third frontal convolution alone is diseased? Those very facts that appear to show that the third frontal convolution is the centre of speech are decisive against the view that it is the island of Reil which is that centre. So that those facts which seem to show that the seat of the faculty is in one or in the other of these places are antagonistic to the other supposition. The facts relating to the third frontal convolution are in absolute contradiction with those relating to the island of Reil, and *vice versâ*. Each series of facts destroys the value of the other. And I may say that, if I had no other argument, I would be contented with that one to show that, when

speech disappears from disease in one of those places, it is not owing to the destruction of the organ of speech, but owing, as I maintained in the two preceding lectures, to a mechanism exactly similar to that which causes paralysis, anæsthesia, amaurosis, and, after all, any other form of diminution of activity of the brain— namely, an irritation starting from the place where the disease is, and propagated to the real centres—never mind where those centres are—whereby their activity is stopped.   My own view, as I shall have to say with more detail in a moment, is, that each function of the brain is carried on by special organs, but that those organs, instead of being composed of cells forming a cluster or mass in one part, are composed of scattered cells diffused in many parts of the brain, in communication, of course, one with the other by fibres, and forming a whole by this union of fibres, but still so diffused that a great many parts of the brain—I would not be bold enough to say all parts—contain the elements endowed with each of the various functions that we know to exist in the brain.   I shall return, if I have time, more fully to that point by and by.

I pass now to another argument against the views of Broca, Sanders, and others.   One series of facts is, indeed, exceedingly striking.   Of all the parts of the brain which have power to pro- duce aphasia, the posterior lobes are particularly remarkable. They are very distant from the island of Reil and from the third frontal convolution—in fact, it is evident that no part of the cerebrum is more distant from the third frontal convolution than the back part of the brain.   These posterior lobes have been, more than any other part of the brain, found to be frequently diseased in cases of aphasia.   Unfortunately, however, a great many of those cases are valueless, for the reason that, although disease was evidently found in the posterior lobe, and no disease was apparent to the naked eye in those two pretended organs of speech that I have mentioned in the front and middle parts of the brain, there had been no micro- scopical examination, and the friends of the localisers, and the localisers themselves, had a very good means of getting out of the difficulty by saying that there was disease in the third frontal convolution or in the island of Reil, but that it had not been found, because a microscopical examination had not been made. This is quite possible; but it is only a supposition, and I know that, in one case in which this argument of a microscopical exami- nation not having been made was brought forward, it happened

that the brain had been kept in Müller's fluid, and was found to be in very good condition for examination, and upon the third frontal convolution, the island of Reil, and all the neighbouring parts (to destroy absolutely any doubt on the point) being examined with the microscope, nothing whatever was found to be altered in those parts. That was a case reported to the Paris Biological Society by a very able young man, Dr. Troisier. There are now several other cases in which microscopical examinations have been made, either on the fresh specimens or after proper treatment by fluids, to render the tissues hard so that they could be cut, and in which nothing has been found in those two organs which are considered as the centres of speech, so that there are now cases—not a great many, but at least eight or nine—in which disease was found in the posterior lobe, and in one of them just at the most posterior part of it, without any alteration of the two pretended organs of speech, and in which aphasia appeared. But as regards those cases in which no autopsy was made, there are arguments which go strongly to prove that, in some, at least, of those cases, the demonstration against the view of the localisers is very good.

These arguments are the following:—In some of those cases the cause of the alteration in the posterior lobe was the obstruction of the posterior cerebral artery. That was the only artery obstructed; and the phenomena at the time the loss of speech occurred being those which we know to follow embolism and softening, it was naturally to be admitted that the only disease was that which was found. It is so also in some cases in which hæmorrhage took place in the posterior lobe. All the phenomena which are so well known as occurring in such cases of hæmorrhage occurred simultaneously; and along with these ordinary phenomena aphasia appeared. The patient having died, it was found that there was a hæmorrhage only in the posterior lobe. It is then exceedingly likely, at least, that what gave rise to the aphasia was the same thing that gave rise to the other symptoms that were observed, and that was the hæmorrhage. Now there are facts with similar features, showing that, in cases of disease of almost any other part of the brain, there has been aphasia. The disease in those cases, as well as in those relating to the posterior lobe, was in the left side of the brain, which is considered as the side possessing the faculty of speech. In a number of those cases the demonstration from subsequent microscopical examination was given, as well as that afforded by the appearance of aphasia, concurrently with the

production of the symptoms produced by hæmorrhage; so that we
have decisive grounds for admitting that almost any part of a
certain dimension on the left side of the brain is capable of pro-
ducing aphasia.

If we followed the mode of proceeding of the localisers, these
facts would lead us to say that, in certain individuals, the centre of
speech is in the posterior lobe—that in certain other individuals it
is in the very front part of the anterior lobe—that in other indi-
viduals, according to a most excellent case of a friend of mine, it is
in the top part of the middle lobe, very near the longitudinal
sinus—that in other individuals it exists in the corpus callosum on
the left side (there are two such cases)—that it exists also in the
corpus striatum, in the optic thalamus, and in the pons Varolii,
and on this last point indeed, if I had time, I should have a good
deal to say. Loss of speech due to disease occupying one half of
the pons Varolii is at present always considered as being due only
to the lack of the power of moving either the tongue, the lips, the
larynx, or the muscles of the chest as freely as is necessary for
speech; and it is said that in those cases the loss of speech depends
generally, if not always, upon the lack of articulation. This is not
correct. There are cases on record, and a good many of them, in
which there was no lack of articulation at all, although the pons
Varolii was considerably injured in one of its halves; and in one of
those cases in which there was distinct aphasia, Cruveilhier, a most
excellent observer, found disease of the pons Varolii, of the medulla
oblongata, and, indeed, disease extending to the part of it in which
anatomists now know that the ninth pair of nerves—that is, the
motor nerves of the tongue—take their origin, without, however,
any defect of motion of the tongue, showing that the centre of that
ninth pair of nerves had not been destroyed. In that case there
was aphasia, and at the same time no difficulty of articulation; so
that there can be—and many cases establish it—disease in at least a
great many parts of the left side of the brain without any alteration
in the third frontal convolution or in the island of Reil, and at the
same time aphasia.

But this is not all. I said in the beginning of this lecture that,
in left-handed people, when there is aphasia, we find disease in the
right side of the brain; but there are so many cases in which disease
of the brain on the right side has caused aphasia, that certainly
we cannot suppose that in all of them the individuals were left-
handed. I have here forty-two carefully analysed cases—and these

are not all the cases of the same description that I have collected—in which there was aphasia, and at the same time no disease save in the right side of the brain. In two of them the individuals were known to have been right-handed; and, with respect to the thirty-eight or forty others, although there is no mention of the fact, I cannot believe that all of them were left-handed persons, on account of the scarcity of left-handedness. These cases are certainly too many—and they are only a part of those on record—to allow the supposition that in all there was left-handedness, and that such was the explanation of aphasia caused in them by disease of the right side of the brain. Besides, in none of those cases was the third frontal convolution found injured, so that the lesion was seen with the naked eye. The disease was in other parts of the brain. There are, therefore, a number of cases which, owing to the seat of the disease, clearly show that the localisation of the faculty of speech in the two places I have so often named, and in the left side of the brain, cannot be maintained as established at all.

I pass now to another series of proofs. Aphasia may appear by attacks, just as I said is the case with respect to anæsthesia and amaurosis in the two preceding lectures. It may appear by attacks, although caused by organic disease, that is, by a permanent cause. How could that be if the destruction of the organ of the faculty in the brain—which in some cases was the third frontal convolution, and in others the island of Reil—was the direct cause of the aphasia? How could those parts be the organs of the faculty of speech, if, after they had been destroyed, the patient lost speech only temporarily, recovering it soon, and then again and again losing and recovering it? How, in other words, could seizures of aphasia come and go like those of epilepsy if those parts are really the centres of the power of speech? It is clear that very frequently they are connected with speech so far as this, that an irritation in them or in their neighbourhood can produce aphasia; but it is evident, also, that they are not the centres of speech, for, if they were, the aphasia should not disappear after having been caused by destruction of those parts. The fact of its coming on is interesting, as showing that those parts have really a connexion with the faculty of speech; but the facts I have mentioned forcibly lead us to admit that it is through an action on other parts of the brain that aphasia appears when there is disease in the two organs I have frequently named. Concluding that, because aphasia comes from an irritation of those organs, that they are the centres of the faculty of speech, is

doing just the same thing as we would do if we concluded that the
soles of the feet are the centre for the emotional power of laughter,
because we laugh when the soles of the feet are tickled.

In other cases, instead of the fit of aphasia lasting for a short
time, followed by a period of normal state, and then by a recurrence
of the fit, there is a long persistence of the aphasia, and then its
sudden or slow disappearance, although the organic cause of it persists.
There are now a number of cases on record in which aphasia has ap-
peared either from disease of the third frontal convolution, or of the
island of Reil, or of both, or from disease of other parts of the right
and left side of the brain ; and this aphasia, after having lasted some
time, and sometimes, indeed, a very long period, has disappeared,
slowly and gradually in most of the cases, but in some cases very
suddenly.   And the autopsy afterwards showed that the organ
diseased had remained so, and that in some of those cases there
was actually a real destruction of the entire organ.   In one case,
given by Dr. Foville, the third frontal convolution and the island
of Reil had been destroyed, and the patient had been struck with
aphasia, and, after remaining so for some time, recovered his speech
completely ; so that, notwithstanding the persistence of the lesion,
the return to the normal state has taken place.   I will, in a
moment, examine the suppositions made to explain those cases.

There is also another kind of case in which the disease, instead
of being in the brain itself, can be in almost any part of the body,
but chiefly in some part of the periphery of the nervous system,
especially in the bowels.   There are very clear cases of aphasia
that depended on reflex irritation of that kind.   Usually, how-
ever, it is not aphasia which comes when there is irritation of the
bowels.   The kind of affection of speech in this case is a loss of
the action of the vocal cords—it is aphonia, and not aphasia ; but,
although aphonia is far more frequent, there are clear cases of
aphasia connected with peripheric irritation.   According to my
views, when aphasia appears in such conditions, it is identically the
same thing as when it is caused by disease either of the third frontal
convolution or of any other part of the brain.   In that case I have
been led to consider that the alterations of speech are produced by
a similar mechanism to that which I have already described; there
is an irritation starting from some part of the bowels, as in cases
of brain-disease causing aphasia there is an irritation starting
from some part of the brain, and in the two kinds of cases the
irritation is propagated to the cells which are endowed with the

faculty of speech—wherever they are—and the action of those cells is inhibited, arrested, or suspended, so that the function is lost.

There is another series of proofs of extreme importance. Take a case which seems decisive in favour of the view that the third frontal convolution is the seat of the faculty of speech. Take a case of disease of the third left frontal convolution, and let the patient be attacked with delirium. He has not said more than one word, or a few words, and always the same; for, as you know, in aphasia, the mechanical faculty of speech is not lost, and the patient can distinctly articulate—sometimes very clearly indeed—one, two, three, or even ten words or more, but the power of speech does not extend beyond that. Let the patient, then, be delirious, and he may talk with the greatest loquacity, just in the same way as a patient who is not aphasic. I am far from saying that, in all cases in which there is delirium in aphasia, he will speak out. In some cases the delirium is the quiet one we observe in persons not aphasic. Sometimes there is that extreme loquacity, rapid in the utterance of words having more or less sense, which we observe in certain cases of delirium. Is not that a clear proof that the faculty of speech is not lost—a clear proof that it was only in abeyance at the time that there was no delirium? Are we not, then, to look upon the activity of cells possessing the faculty of speech as being simply arrested, simply momentarily stopped, just as the heart's activity is stopped when we galvanise the par vagum? Is not that a clear proof that, notwithstanding the destruction of the pretended organ, the function can continue? which implies that " the so-called organ " is not the organ, or, at least, that there are other organs of speech elsewhere?

Another fact was communicated to me by a gentleman connected with me by marriage. He wrote to me about his father (a physician of eminence), whom I had seen, and who was aphasic. He said—" I noticed what seemed to me to be a most singular thing about him last night. I was staying with him, and he slept quietly until about two o'clock, a.m., when he commenced talking in his sleep. Every word was as distinct and clear as in health; there was no mumbling or miscalling of words, as was the case always in the daytime when he spoke at all." And then he adds—" His ideas were apparently connected, and he certainly was dreaming, but, when he woke up, there was no remembrance of what had occurred." So that, both in delirium and dreaming, the faculty

which is stopped in aphasia can find vigour again and manifest itself. In that case, unfortunately, no autopsy was made, so that I do not know where the lesion was. At any rate, the patient had symptoms which seemed to show that the third frontal convolution was diseased—he had elevation of temperature of the left side of the head, local pain under the left temple, paralysis of the right limbs, and a number of other symptoms which seemed to indicate that the disease was at the level of the third frontal convolution, or very near it.

I now pass to a series of entirely different arguments. This series of proofs is taken from negative cases—cases in which the pretended organs of speech have been destroyed without aphasia, and of which I must say there are a great many. I have notes of a very large number of cases in which the third frontal convolution, or the island of Reil, or both, or the whole anterior lobe, or the whole middle lobe on the left side, has been destroyed, and that without any appearance of aphasia, or with only a slight difficulty of speech. The number of cases now in which such a thing has been observed is really considerable; and there cannot be the least doubt that the pretended organs of speech on the left side of the brain in right-handed people have been fully and completely destroyed, and not only those parts, but also much of the neighbourhood—which makes sure, of course, that those parts, at least, were fully destroyed—and yet in those cases aphasia has not appeared.

This is certainly, I think, a decisive proof against the correctness of the view I am opposing; but it is hard sometimes to beat out a hypothesis when it has taken hold of certain minds. Many medical men have tried to explain away those cases which are in opposition to their views.

The first thing that has been said is, that, as we know that a child possesses two brains, but that only one of these responds to the education of speech, if that one becomes diseased, the other may then develop the power of speech. It has been supposed that, when the left side of the brain has been diseased, and no aphasia has appeared, although there is no left-handedness, it may be because the right side was the organ of speech in those individuals. Another supposition which has been put forward is, that, in those individuals who have been cured after having been aphasic from disease in the proper place, according to the localisers—namely, on the left side of the brain—that organ on the right side has grown gradually, after the loss of power belonging to that of the left side,

and has assumed the lost function, just as a person who has lost the right hand comes to be able to write with the left. Certainly it is possible that it is entirely or partly so. I do not deny that both of those two suppositions are possible. But there are cases, unfortunately, which leave no doubt that, if the supposition be true that the faculty of speech belongs to the third frontal convolutions on the left and on the right sides, we should see a persistent aphasia appear from a destruction of tissue, including those organs on both sides. But it is not always so. In one case, published by Professor Jacquemet, of Montpellier, in which the man had a fracture of the frontal bone and of the neighbouring bones, and in which there was a crushing in and destruction of the two sides of the brain in those parts which are said to possess the faculty of speech, there was never any alteration in the man's speech. A number of such cases are on record. I shall elsewhere quote a good many of them; but I must mention, at least, a remarkable one given by Dr. Delpech, in which there was destruction of the two anterior lobes without any alteration of speech. Professor Velpeau, in whose wards at the Charité Hospital the case was seen, related it to the Academy of Medicine, and claimed to have won by it a prize which had been offered by Professor Bouillaud for the production of any such case. In that case the two anterior lobes were replaced by an immense cancer extending back to the middle lobe, and which certainly must have destroyed the pretended organs of speech on the two sides. So that there are cases on record in which the supposition of compensatory action making the right side of the brain to act in place of the left cannot be held available.

RECAPITULATION.

I will not insist more on this point now, as time presses, and I must give the general conclusions of the three lectures which I have delivered here.

There is certainly, as regards anæsthesia, as regards amaurosis, and as regards aphasia, evidence that any part of the brain can produce those symptoms. On the other hand, there is evidence that great masses of the brain on either side can be destroyed without any marked alteration either in sensibility, in power of sight, or power of speech. There is, therefore, a complete demonstration, by these two series of facts, that the localisations of those various faculties in the brain are to be rejected. And we are led

to admit, therefore, in the first place, that if there is any localisation
at all—and I admit it fully—it must be of the kind I have men-
tioned—namely, that those faculties are localised in cells which are
scattered; so that, if in an animal, for instance, we take away the
brain from the front towards the back, layer by layer, we find that,
if loss of sensibility comes on, it is only at a certain moment, when
we have taken away a very great part of the brain. So it is with
respect to the organ of sight. It is interfered with only to a
certain amount, when we reach a certain part in the back. If we
begin by taking away part of the back and top, we can go forwards
and downwards to a certain point without any marked effect, and
then suddenly the symptoms will appear. I need not say that we do
not find aphasia, for we cannot, of course, find the loss of a faculty
that does not exist. But, as regards the two other faculties of
sensibility and sight, we see them disappearing in the way I have
said—that is, not gradually and slowly, but, on the contrary,
suddenly or rapidly. It seems, therefore, that the nerve-elements
endowed with those faculties are spread through and are present
in almost every part of the brain.

There is one fact on which I should have dwelt in the two
preceding lectures, as it might have rendered clearer what I said.
That fact is that, in every case that we study carefully, in which
there is disease limited only to a small part of the brain—that is,
to those parts which may be considered as the centres or conductors
chiefly, or exclusively, of the perceptions in question, and in which
there is amaurosis in a complete degree in one eye, or in both, or
complete anæsthesia in one half of the body, there is proof that,
when only a part of their fibres, or of the cells that serve to the
function, is injured, we find that the faculties are lost, notwith-
standing the persistence in a normal state of only a part of the
organ serving to it. We must, therefore, admit that it is not
owing to the loss of function of the diseased parts that the symp-
toms appear, and that there must be another mechanism causing
the disappearance; and if we take the other fact, that the con-
ductors or centres can be considerably altered without any symptom
whatever, we find there also something leading to the view that it
cannot be owing to the destruction of the tissue that the appearance
of the symptoms, when they do appear, is due, but that there must
be some other cause for it. What is that other cause if it be not
the irritation? Irritation, as you well know, can produce most
wonderful effects. Take, for instance, those admirable facts,

asserted by Dr. Bochefontaine, of Paris, showing that every organ in the abdomen and in the chest can be influenced, and influenced immediately, by the galvanisation of a small part of the brain; and take the other facts adduced by the same physiologist, which show so remarkably that the irritation of sensitive nerve-fibres near the brain, but out of it, in the dura mater, can produce movements similar to those we find when we galvanise the brain itself. If you tickle the dura mater with your finger-nail, the anterior limb will jump just as it would if you galvanised the pretended psycho-motor centres of the arm. If you tickle another part of the dura mater the posterior limb will jump also. So that if we were to conclude as Fritsch and Hitzig, and almost all the physiologists after them have done, we should say that a certain part of the dura mater is the motor centre for the movement of the anterior limbs, and that another part of it is the motor centre for that of the posterior limbs.

Take also an experiment of my own, which is extremely remarkable, and was shown, like many others, to the Biological Society of Paris. To facilitate the operation, anaesthesia was produced by the injection of a dose of chloral into the veins of a dog, on which the upper surface of the right side of the brain was cauterised with an iron heated to white heat. As soon as the animal recovered from anaesthesia he showed all the symptoms of meningo-myelitis in a most intense degree in the hind legs and the lumbar part of the trunk. He was shown to the Biological Society about three or four hours after having recovered from anaesthesia. The symptoms were quite characteristic. My friend and colleague, Dr. Hayem, who has studied meningo-myelitis with great care, exclaimed, "Your dog is attacked with it." I said that an autopsy would show that there was no such inflammation, as I had already made autopsies in nearly similar cases without finding the cord and its membranes inflamed. An autopsy was accordingly made, and no appearance of inflammation was seen—there was congestion, but nothing else—but the cells of the gray matter of the dorso-lumbar enlargement of the spinal cord had evidently been acted on by irritation of the brain, and their activity had been morbidly altered and considerably increased, so as to produce the phenomena observed, which were exactly those we find in genuine cases of meningo-myelitis. In other cases the same burning, instead of producing a morbid activity in the cells of the spinal cord, arrested, or, if you prefer, inhibited the normal activity of those microscopic

nervous centres, and produced a slight paralysis—sometimes on the same side, sometimes on the opposite side, but generally more marked on the opposite than on the corresponding side. So that, by those experiments, I found something similar to what we see in cases of brain-disease in man—that is, that an irritation of the surface of the brain can produce two things—namely, engender a morbid activity, or put in play the normal activity of the nerve-cells in the spinal cord, or arrest, destroy, inhibit that normal activity.

These are the two series of symptoms that we find in cases of disease of the brain. We find a morbid activity or increase of the natural activity, and we also find just the reverse—namely, a cessation of activity. If we take the case of cessation of activity, we find that it must depend on something similar to the arrest of activity of the cells of the heart when we galvanise the par vagum. It is very difficult for persons who have not reflected on the point and studied the cases, some of which are favourable to the view in question, while the others have no bearing on it, to admit that the activity of nerve-cells will be stopped, just in the same way as that of the cells of the heart, by an irritation in some part of the brain, so that this arrest of activity will persist for years and years sometimes. But we see, among other facts, which are certainly quite in favour of the view I hold, that sometimes, in cases of disease of the brain, the heart's action itself is considerably diminished or partially stopped for many days consecutively. There are cases on record in which, for seven weeks, the heart has remained beating only between 35 and 45 times in a minute; so that the inhibitory influence exerted on the heart can persist for a very long time indeed; and it seems that inhibitory influences causing paralysis, anæsthesia, amaurosis, and aphasia, can be produced in the same way; and we know that the arrest of the heart's action can stop suddenly in those cases, as, indeed, sometimes happens in those cases of fainting when there is disease of the brain, and when the heart returns to a normal rate of beating or to a state of great activity; and, in the same way, we find that, in rare cases, but, at the same time, very clear and positive ones, which have been well studied by medical men, there has been a sudden return of sensibility, of sight, or of speech. And, as regards sensibility, the fact discovered by my friend, Professor Vulpian—that, in cases of organic disease of the brain, it not rarely happens that galvanising the skin will cause sensibility to return on the side

on which it had disappeared—is very much in favour of the view that the power of feeling was only kept in abeyance, was only inhibited, stopped, or arrested by au action which the galvanism destroyed.

THERAPEUTICS.

I do not yet know whether efforts have been made to cure aphasia or amaurosis by galvanism. Of course it would be well worth the trial. I have no great hope, however, that such a means will be successful. However, it is desirable that the trial should be made, for the point is one of great importance. The current to be employed should be of that class which acts on the skin with great power, and many shocks should be given in rapid succession. In fact it should be the same kind of current that is found most effective against neuralgia. In making use of the current to obtain a decided action on the skin, as you well know, the terminal part of the conductors must be dry, and the employment of a metallic surface is almost essential.

But it is not by galvanism only that we may have a chance of producing great effects by irritation of the nerves of the periphery, so as to do away with the action—whatever it is—which stops the activity of the nerve-centres. If we employ means such as the actual cautery, in a way I will presently explain, we may have a good chance of doing service; and I have some very good cases showing the effects of that curative agency, having myself frequently employed the actual cautery. The actual cautery may be used so as not to give rise to more than very little pain; and it may be used with more power than if applied in such a way as to give a great deal of pain. Also it may be applied so as not to produce a sore, or to give rise to marks or cicatrices that will last— an important point, if the place of application of the cautery be the face or neck. A brownish tinge of the skin, lasting for five or six weeks at the most, is the only mark that will be made. The instrument should have the shape of one of the points of a small olive. It should be used only when at a white heat, and then passed very rapidly over the skin; and the frictions should be made with some firmness at the proper place.

In cases of brain-disease, the best point of application is the head itself, or the lower part of the neck and the interval between the shoulder-blades. These applications should be made five or six times, but not all at once, for then the instrument would have

time to lose the proper temperature. If used after its temperature has sunk from a white to a red heat, it would give rise to great pain and to sores.

Long ago, when I was quite a young man, I studied in the wards of the Charité Hospital, in Paris, the comparative effects of cauterisation with the white and the red hot iron; and I found that the white, though so little painful as compared with the other, was by far the best.

This application has really far more power in improving the condition of the nervous centres than any other means that I know of. Applications in the place I have indicated are the best, but sometimes we have to make them on the head itself. In fact, in cases of diffused cerebro-meningitis and general paralysis of the insane, a great deal of good can be obtained from it; and indeed, as far as diagnosis can be complete where there is no autopsy, I believe that a patient of mine, a physician in New York, has been cured by this means.

Amongst other means, the use of ice ranks, I think, next to the actual cautery in point of power. It should be applied either by friction or in a pounded form; but I do not recommend Chapman's ice bag, because the action of ice is there too much diminished. The bare ice on the skin or an application of pounded ice in thin linen, for ten or fifteen minutes, on the back of the neck and head, the latter being shaved, is most useful.

Amongst internal means one of the most powerful is strychnine, but it must be pushed to the extent of producing stiffness. If you deal with paralysis, the stiffness must be maintained for a whole month. You must take care to give just the dose that will produce that effect. There is not the least danger in this course, if you watch carefully over the effect produced, modifying the dose according to what you observe. I myself was very timid about it for a long time, and then had no very good results from the use of the remedy. When Professor Fouquier, of Paris, first tried the use of nux vomica in paralysis, and sometimes obtained very great success, he was exceedingly bold, and produced those spasms very violently, but did not mind it. Strychnine, in cases of amaurosis due to brain-disease, when there is no affection of the eye itself, would certainly prove useful, as well as in those other kinds of amaurosis in which its use is so well-known to be of great value. I have already found it useful in some of the cases I have attended.

CONCLUSION.

I will not insist any more on these points, as time does not permit. I have now to thank the President and the College, who have given me this opportunity of lecturing before an audience so extremely attentive as you have been. I thank you very much for your kind attention to me.

ART. IX.—*Cases of Traumatic Tetanus; with Observations upon the Treatment of this Disease.*[*] By HENRY FITZGIBBON, M.D.; Surgeon to the City of Dublin Hospital.

CASE I.—John Nolan, a strong healthy-looking boy, aged about twelve years, presented himself at Clondalkin Dispensary, on Wednesday, the 24th of February, 1871, having walked a distance of over two miles, along with his mother, for advice; the tetanic expression of his face was so well marked that the first glance satisfied me that he had lock-jaw. His mother stated that on the previous morning he complained of severe pain in the small of his back; towards evening she perceived "a queer look in his face," which increased during the night; and as he now complained of difficulty in swallowing, she had become alarmed. It had not occurred to her mind that he had got lock-jaw, as she was positive that he had received no injury or fall that could induce it, nor had he received any severe wetting or been exposed to any unusual cold. On making more careful inquiry, I elicited from her that on the previous Sunday he had come home very much heated from playing hockey with a number of other boys; in the evening he complained of his feet being sore from having worn a new pair of wooden clogs while playing; this was the only exciting cause I could discover. I made a minute examination of the boy; he also said he had received no hurt or fall; the tenderness of his feet had gone away; he had walked to the dispensary in the same clogs without any discomfort; there were no bruises on his feet or any part of his legs or body; he complained only of pain in his back and a difficulty in swallowing.

He was put to bed on his return home, his parents refusing to take him to hospital. Having recently read the report of one or two cases of tetanus which recovered under treatment by hydrate of chloral, I was induced to treat him with that drug, which was

[*] Communicated to the Dublin Biological Club, Tuesday, February 13, 1877.

accordingly given in gr. x. doses in water every hour until it produced sleep, and to be repeated in the same manner upon his awaking. He was given as much fluid nourishment as he could take in the form of beef-tea, eggs, &c. He slept quietly after the second dose of the chloral, for more than two hours, but awoke in a violent tetanic spasm; the chloral was continued steadily, but the disease rapidly increased. On the 26th of February his whole muscular system was rigid; he had constant opisthotonos, and frequent very violent spasms, especially at the time of awaking. His parents conceived the idea that the medicine was making him worse, and refused to continue to administer it any longer. He died upon the sixth or seventh day from the appearance of the first symptoms of the disease. The muscular spasm in this case was general from a very early period. There was violent opisthotonos, but no pleurothotonos or lateral tendency whatever; there was obstinate constipation throughout.

CASE II.—Felix Kearns, a slight but healthy looking lad, aged seventeen years, a labourer, presented himself at the dispensary of the City of Dublin Hospital on the 24th of January, 1876. On the previous day he had received an injury of the right hand, by the falling of the side-board of a cart, which came down catching his hand with a chop upon the wheel. There was a lacerated wound across the back of the hand which exposed the extensor tendons, severing completely that of the extensor communis digitorum to the third finger, the power of extending which was lost. The wound was carefully cleaned with carbolic acid lotion, drawn together with plaster, and covered with a compress of lint soaked in carbolic oil. He kept the wrist flexed, but had the power of extending it at the time of his first visit to the hospital; but on the 27th of January, as he was unable to do so, it was placed upon a splint. The dressing with carbolic oil was continued, and the wound appeared to be healing kindly, without any bad symptom, up to the 6th of February. On this day, which was the 13th after the accident, he eat his breakfast as usual, being in good health but for the injured hand, and went out immediately afterwards.

The morning was frosty and the day chilly and damp, sleet fell in the afternoon, and as he remained out the entire day, he returned home both wet and cold. On his return his mother noticed " a queer look in his face;" he complained of a pain in his back and difficulty in swallowing.

He was admitted into hospital the following morning, the tetanic symptoms having developed themselves very rapidly. His teeth were now clenched; he could not protrude his tongue in the least; he complained of severe pain in the back; all his muscles were rigid, but he had no marked spasms; the risus sardonicus was extreme. The wound was clean and healing well; he referred no pain or uneasiness to it. His bowels were constipated, and had been so for several days. His pulse was 72; his temperature 99·5°; respiration 26.

The splint was removed from the arm, when the wrist and elbow became forcibly flexed, and the arm pronated; the wound was now dressed with equal parts of tincture of belladonna and tincture of opium. A copious enema of more than a pint of warm water with two ounces of castor-oil, was administered; he was ordered gr. ¼ of ext. of belladonna in pill every hour. An ice-bag was applied to the back of the neck and upper part of the spine for half an hour, and re-applied every third hour for the same period during the night.

Feb. 8th.—Had passed a good night; pulse 82; temp. 99·5°; respiration 26; could suck fluids through his teeth, but had a disinclination to take nourishment, owing to the difficulty of swallowing. The large enema given the night previous had been retained, although he passed urine frequently. The pupils were fully dilated, and he complained of dryness of throat. He was given milk and strong extract of beef frequently, but seldom could swallow more than three or four teaspoonfuls at a time. Another enema was given, consisting of castor oil ʒi., tr. assafœt. ʒss., turpentine ʒss., water ʒii. As no discharge came from the bowels for several hours, and he said he could not relieve himself unless he was taken out of bed, he was with difficulty, owing to his rigid state, placed upon a night chair, when a free evacuation took place, from which there was no unusual fœtor, nor was it in any way remarkable except in quantity.

Feb. 9th.—Suffering greatly from the violence of the spasms, crying out that his back was breaking; pulse 90; temperature 100°; respiration 26. Belladonna continued and also the ice-bag to the spine.

11th.—Fifth day of the tetanus; symptoms greatly worse, with profuse sweating, weak pulse, and a miliary eruption all over the body.

12th.—Symptoms generally worse; screaming on the approach of each spasm, with marked pleurosthotonos to the side of the injury,

the spasms appearing to commence in the wounded limb, and to continue longer and more violent in it than in the opposite one. The belladonna was continued; in addition, he was ordered tobacco stupes to the belly.

13th.—He had had a motion from his bowels unconsciously during the night, but there was no change in his condition since the previous day. The belladonna was continued in $\frac{1}{4}$-grain doses of the extract every second hour, and he was stuped three times during the day with tobacco.

14th.—Had a bad night; hearing morbidly acute; violent spasms excited by the slightest noise. The treatment was continued with the addition of an enema of an infusion of 20 grains of tobacco at night. This was followed by some alleviation of the spasms, but the patient did not sleep.

15th.—The belladonna, tobacco stupes, and ice-bags were continued. He was also given 25 grains of hydrate of chloral at night. He slept for three hours from the effect of it, after which he awoke with the most violent spasm he had yet had, during which he appeared upon the point of death from asphyxia, when the resident pupil on duty, Mr. Edmund Rundle, with great promptness, put him under the influence of chloroform, which most probably saved his life. He slept again after this for some time.

16th.—At 8 a.m. this morning he had another spasm, which appeared to threaten his life, during which he expelled a considerable quantity of bloody froth from his mouth and nostrils. In the afternoon, as he had not slept, he was given 25 grains of chloral hydrate. It was followed by a short sleep, from which he awoke in a very violent spasm. He was given the same quantity of this drug at 6 p.m., which was followed by another short sleep, and a paroxysm of unusual violence upon awaking, so that the further administration of chloral was now forbidden, the belladonna, tobacco stupes, and ice-bag to the spine, being continued.

Upon the 18th February, which was the twelfth day of the tetanus, another spasm of such violence occurred that chloroform was again resorted to, and appeared to avert a fatal termination.

19th.—There was more bloody froth expelled from the mouth; the belly was tympanitic and tender to the touch; the bowels had not been moved for ten days; 5 grains of calomel were given; in a few hours this was followed by an enema of castor-oil and tincture of assafœtida, which acted well, after which the distension of the abdomen subsided, and the tenderness disappeared. He seemed

to mend from this period; and although quite rigid, with constant opisthotonos, and pleurosthotonos towards the injured side, he suffered from no further very violent paroxysm.

On the 21st the ice-bag made him uncomfortable; its use was therefore discontinued.

22nd.—Profuse, constant sweating had set in without any further improvement in his general condition.

23rd.—Sweating continuously ; pulse 120; very weak; temperature 98·5°; respirations 26; the tetanic rigidity undiminished.

24th.—Still perfectly rigid. There was constant priapism, and he complained of retention of urine for the first time, and sweating to an extent that was awful. In this condition he was put with difficulty into a warm bath, temperature 100°. He was like a wooden figure when placed in the warm water; but in a few minutes his whole muscular system became relaxed in a manner that it had not been from a very early period of his illness. He passed urine freely, and felt such relief and comfort that he cried out repeatedly "this is grand." Having been fifteen minutes in the bath, he was placed upon warm sheets, carefully dried, and put to bed, when he immediately fell into a quiet sleep, which continued for three or four hours.

25th. There was a marked improvement in every respect since the warm bath—his pulse being 100, temperature 98°, respirations 24, sweating but slightly, able to protrude the tongue about half an inch, and wishing to eat an egg. From this period he steadily improved, getting a warm bath every second day, the use of belladonna being continued, so as to keep the pupils fully dilated. He was discharged from hospital, perfectly well, early in March.

Case III.—William Kelly, aged twenty-one years, by occupation a baker, was admitted into the City of Dublin Hospital on the 24th of August, 1876, having received a severe injury to the left hand and arm, by its being drawn into a pair of biscuit rollers. Previous to the accident he had been generally of temperate habits and had enjoyed good health. When brought to the hospital he was pallid from loss of blood and shock. There was a lacerated wound extending from the base of the index finger to about three inches above the wrist-joint, the radius and ulna appeared to have been forced asunder by the pressure of the rollers, causing rupture of the annular ligament and the anterior radio-ulnar ligament, and contusion and laceration of all

the other soft parts, so that the hand and wrist were regularly
burst open, exposing both the superficial and deep flexor tendons
at the bottom of the wound. The arm, to within a few inches of
the shoulder, was so contused that it appeared almost hopeless to
attempt to save it. The hæmorrhage was profuse. Having brought
the palmar rent together, and replaced the lacerated tissue, and
secured the parts with eight or ten long pins and a continued
suture, the whole limb was dressed with carbolic oil. This was
not removed until the third day, when the limb had become so
inflamed that it appeared almost necessary to make incisions in the
upper arm to relieve the tension. The carbolic dressing was changed
for lotio plumbi acetatis, except at the laceration, which was again
dressed with carbolic oil. On the fifth day the inflammation had
greatly subsided, the pins were removed, and the hand bandaged
with carbolised gauze. Up to September 8th, which was the tenth
day after the accident, all went well. He now complained of loss
of appetite and constipation, his tongue being slightly coated.
He was given 3 grains of calomel, which acted upon his bowels,
and these symptoms had disappeared next day.

September 16th.—Complained of having had a sleepless night,
which he attributed to the recurrence of constipation. He suffered
from pain in the back, and said his jaws were stiff, his throat sore,
and that he had difficulty in swallowing. There were twitchings
in the wounded arm and general malaise. The risus sardonicus
was marked, the sterno-mastoids and muscles of the abdomen were
tense. Any attempt to examine his throat by depressing the
tongue brought on a paroxysm of trismus, which clenched the
teeth firmly together. His temperature was 100°; pulse 110. He
was given 5 grains of calomel, and ordered ¼ grain extract of bella-
donna every sixth hour. The wound was dressed with a lotion
containing equal parts of tincture of belladonna and tincture of
aconite.

17th.—Still rigid and teeth clenched; the bowels had acted; the
pupils well dilated; slight dryness of throat; temperature 99·8°;
pulse 90.

18th.—Temperature 98. In other respects no change for the
better. There were frequent twitchings of the wounded limb, and
the process of dressing it excited painful spasms in it. The local
application of belladonna and aconite, with the belladonna internally,
was continued daily without any marked change up to the 28th of
September, when the trismus and general tetanic condition appeared

somewhat less. On this day he was given a dose of castor oil, which acted well; and the following day his condition was so much improved that he eat a mutton chop, which was the first solid food he had made use of since September 16th. From this date he steadily recovered. and lost the expression of face sometime before his discharge from hospital, which was on November 5th. At this time he was in perfect health, but for an unhealed superficial ulcer upon the upper arm, where the skin had sloughed extensively from the friction of the rollers, his wrist also being permanently stiff.

The want of a certain knowledge of the pathology of tetanus, to indicate the rational treatment of this disease, has led to the empirical use of medicines of the most opposite physiological actions. We have, however—if not positive—stong presumptive evidence that the tetanic condition depends or is, at least, associated with hyperæmia of the spinal cord and medulla oblongata, and in some instances actual inflammation of these parts. Dr. James Copland, in his article on " Tetanus" ("Dictionary of Medicine"), says: "The spinal cord, medulla oblongata, brain, and their membranes, have frequently presented changes more or less decidedly morbid in tetanus and trismus. I believe that these changes are rarely altogether absent, especially as respects the spinal cord, medulla oblongata, the pons Varolii, and their membranes, when the inspection is made within twenty-four hours after death, and when these parts are carefully examined." He adds—"In no inspection which I have witnessed have inflammatory appearances in one or other of these parts been altogether wanting." He mentions the names of Larrey, Magendie, and many others who viewed these appearances as the pathological causes of tetanus.

Clifford Allbutt published (Path. Trans. XXII., 27) notes of four cases of tetanus, with an examination of the spinal cord in all. Lockhart Clarke and Dickinson, report on the same specimens, and the pathological results are as follows:—1. Diminution of consistence of various degrees and situation in the cord. 2. Hæmorrhage in two of them visible to the naked eye. On microscopical examination—1. Great distension of the blood-vessels in both white and gray matter, with occasional exudation and disintegration of tissue around them. 2. Isolated patches of disintegration of various shapes and sizes in both gray and white matter; and in the gray matter numerous vacuities, having, on transverse section, circular

or oval outlines, and resulting from disintegration of the nerve-fibres.

There are many other cases reported, in which the *post mortem* examinations revealed either hyperæmia of the medulla oblongata and spinal cord, or a condition that was probably consequent upon hyperæmia having advanced to actual inflammation of the upper portion of the cord.

Dr. Robert M'Donnell has recently examined the cords of several persons who had died of tetanus, and although no such extensive lesions existed as are stated to have been found by many other observers, yet, under the microscope, the sections presented appearances which probably depended upon distension of the blood-vessels from hyperæmia of the cord in various parts.

That this is the pathological condition of the spinal cord which produces tetanus in many cases, is, I think, borne out, not only by the foregoing, but by every case which recovers. The fact that a great number of cases of the greatest severity have recovered rapidly as soon as the disease began to abate at all, shows that it cannot depend on any serious lesion of substance in the cord or elsewhere, but upon some condition such as hyperæmia, which is capable of rapid subsidence, and which leaves little or no *post mortem* appearances in the majority of cases—unless, as Dr. Copland recommends, the examination is made within a few hours after death.

In the first of my cases the exciting cause was such as would induce hyperæmia of the cord or even myelitis—great mental excitement of the game, over-exertion kept up to exhaustion, very violent perspiration, which was probably injudiciously checked.

A case of intermittent tetanus, which has been reported by Reigle, also appears to me to point to the same cause. The primary source of the disease in this case was the irritation of worms in the bowels; but the tetanic condition could be brought on at once by pressure upon the large vessels and nerve-trunks. The probable result of this proceeding would be hyperæmia of the cord, caused by obstructing the circulation to the rest of the body.

Dr. Charles Philips, in his Materia Medica, states that he has seen three cases of persons who were thrown into a state of " complete tetanic rigidity " by the administration of opium. The well-known antagonism of opium to belladonna makes this fact an additional proof that the physiological action of belladonna is to produce an opposite condition of the nerve-centres from that upon which tetanus depends.

Professor Brown-Séquard writes—"This most powerful remedy has been employed quite blindly in the various forms of paraplegia by the French and Italian physicians."

An eminent author of a very learned work on Therapeutics and Pharmacology, declares that—"It is quite obvious that it (belladonna) should never be employed in cases dependent on congestion, inflammation, or organic lesion of the nervous centres until this condition should have ceased entirely and nothing be left but inertness." The truth is, that it is precisely in cases of congestion or inflammation of the spinal cord or its membranes that belladonna should be used against paralysis. He (Brown-Séquard) sums up the indication for its use or non-use as follows:—"First, belladonna is one of the most powerful and reliable remedies that we may employ in cases of paraplegia with symptoms of irritation of the motor, sensitive, or vaso-motor, or nutritive nerve-fibres of the spinal cord, or of the roots of its nerves; in other words, in cases of congestion, meningitis, or myelitis. Second, belladonna is a most dangerous agent, able only to increase the paralysis if employed in cases of paraplegia without symptoms of irritation, such as cases of white softening or of the reflex paraplegia."

In 1843, a volume, entitled "Posthumous Extracts from Veterinary Records," was published by a veterinary surgeon named Field. This book gives the *post mortem* appearances found in the cases of animals which had died of tetanus. It contains also a report of a case occurring in a young man, which Mr. Field treated by administering extract of belladonna in doses of ⅓th of a grain every fourth hour. The patient recovered steadily under this treatment. It is reasonable to conclude that the selection of belladonna in this case was the result of experience of its effect in veterinary practice.

In thus stating the grounds of my belief—that to treat tetanus by means of belladonna is not merely empirical but based upon a reasonable theory as to the pathology of the disease—I do not, for a moment, mean to imply that all other remedies ought to be discarded for its sake; on the contrary, I think that tobacco, nicotin, curara, cannabis, and chloroform may be valuable in conjunction with belladonna.

Nicotin produces paralysis of the voluntary muscles, which, by relieving the violence of the spasms, gives time for the subsidence of the excited condition of the cord. Curara causes paralysis of the peripheral motor nerves, but has no action whatever on the brain, spinal cord, or its membranes. It, therefore, seems to me that it

would be perfectly rational to combine the internal administration of belladonna with subcutaneous injections of curarine or nicotin; but, as the latter has at first a tendency to produce an excited and irritated condition of the cord before it causes any paralysis of the muscular system, I should be disposed to employ curarine in preference.

Constipation, which is, I think, constant as the result of spasm of the sphincter muscle, is best relieved by calomel and enemata. Warm baths, as in the case of Kearns, may give great relief, and produce marked relaxation of the muscular spasms, and by doing so stop exhaustive sweating, if it depends on active muscular contraction.

The local application of aconite and belladonna to the wound is very important, as it diminishes the irritability of the wounded nerves.

Perhaps the most important point of all is to recognise the premonitory symptoms before the disease has actually declared itself; if we could always do this, I think a great proportion of the cases, which become severe, would run a mild course, as in the case of Kelly.

In all the foregoing cases these symptoms were the same. The "queer look in the face" first caused the mothers to ask if anything was the matter in the first two. And in the third, the brother of the young man told me he had noticed a "frown on his face" before any idea of tetanus had occurred to me or the nurse who attended him. Pain in the back, a feeling of soreness about the throat, and constipation, include all the other premonitory symptoms which existed in these cases before the occurrence of trismus, which at once indicated their true character.

I have not alluded to the use of physostigma, although I am aware of the favourable reports of those who have employed it, because I think its physiological action, in some respects, would render its administration in conjunction with belladonna incompatible. There are, however, quite rational grounds for the treatment of tetanus with this drug. It is known to depress the excitability of the ganglionic elements of the cord; it especially affects the groups of cells in the anterior horns of gray matter; by its action on the gray matter of the cord generally it diminishes its motor and reflex activity. So far the action of this medicine would indicate its use, and that based upon most rational grounds, especially if tetanus depends upon hyperæsthesia of the cord, whether depending on hyperæmia or accompanied by it.

In publishing this paper, I do not wish to appear dogmatically to advocate the use of belladonna in every case of tetanus, because I can conceive the possibility of some cases depending upon an opposite condition from that in which it would be useful, such as arterial anæmia; this, by a venous congestion of the cord and its membranes, might produce an excitement of the motor nerves and produce tetanus; in such a case belladonna, by its action on the vaso-motors, would diminish the arterial supply to the cord and increase the disease.

The constitution of the patient and the previous history of the case must be taken into consideration, and will afford ample means for forming a reasonable opinion as to whether a condition of arterial anæmia or the reverse exists.

To support the strength by every possible means of nutrition, attention to the bladder and bowels, and constant watching, so as to meet every emergency that may arise, complete the rational treat-ment of tetanus.

SUBCUTANEOUS INJECTION OF COLD WATER FOR THE RELIEF OF PAIN, AND ESPECIALLY IN ACUTE ARTICULAR RHEUMATISM.

MANY years ago subcutaneous injection of water was recommended, particularly by Dieulafoy, in rheumatism of the extremities. The injection was inserted in many parts round the affected joint, ten drops being the quantity employed. The results were most remarkable. The pains diminished, and the patients were able to use the affected limbs; nay, even by this treatment alone and after a single operation, the rheumatism was entirely got rid of. Even against muscular rheumatism, sciatica, &c., it has proved to be a means of very great value.—*Gaz. des Hôp.*, 1876, No. 99, and *Centralb.*, No. 53, 30th Dec., 1876.

J. M. F.

USE OF AROMATIC SULPHURIC ACID IN NECROSIS.

THE history is recorded of a case of necrosis of the alveolar process, which was cured by the daily injection of aromatic sulphuric acid, in the proportion of a drachm to the ounce of water, into the substance of the spongy swelling that surrounded the diseased bone. The teeth were very loose, and the case was one that apparently called for operation. Under the above treatment, combined with the use of tonics and an animal diet, the teeth gradually tightened, and in a year the cure was complete.— *Boston Medical and Surgical Journal.*

K. F.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Illustrations of Clinical Surgery. By JONATHAN HUTCHINSON.
Fasciculus VI. London: J. & A. Churchill.

THIS is one of the most interesting and important fasciculi which
Mr. Hutchinson has yet published. It is particularly opportune,
because it deals with a question—vaccination syphilis—which just
now, in the presence of an increasing epidemic of small-pox in
England, is urged upon our attention by the opponents of vaccina-
tion. There can no longer be any doubt that the received mode of
attempting to prevent the dangers of this fever does present some
risks, and, although they are infinitesimal in number, a few
individuals have been poisoned by syphilis through it, and so made
the innocent victims of a horrible disease. Mr. Hutchinson has
been able to collect a number of cases in which this misfortune
occurred to several persons, and the histories which he gives are full
of interest and value. He takes occasion to say that he is never-
theless a firm advocate of compulsory vaccination, and in no way
does he sympathise with the " misguided zealots " who oppose it.

We must refer our readers to the remarks of the author upon
this subject; but we may here briefly summarise some of the more
important views which he enunciates. He thinks we must believe
that the specific poison of syphilis is either not contained in the
vaccine-lymph at all, or is not equally diffused through it. The
supposition that it is necessary to convey some of the cell elements
of the blood in order to convey syphilis, seems to be the most
probable explanation. Probably it is not necessary that these
should be visibly red. It is moreover probable that it is not
absolutely necessary that blood should be used in vaccination in
order to convey syphilis, but that it is quite sufficient to allow the
vesicle of the vaccinifer to drain or weep.

The order of events in such a case is that the vesicle goes
through the usual course and heals. Then at the end of about a
month the syphilitic virus begins to show its effects, and the scar
becomes irritable, inflames, and indurates. It then breaks into an

ulcer, but sometimes takes the form of a red firm glossy tubercle. In all the cases in which mercury was administered the effects of treatment were satisfactory.

The plates accompanying the letterpress are admirably executed, and are equal in all respects to those which have already been issued.

WORKS ON DISEASES OF THE SKIN.

Epitome of Skin Diseases. By TILBURY FOX, M.D., and T. C. FOX, M.R.C.S. H. Renshaw.

PART I. of this little work is almost identical, except for the size of the pages, with the " Key " which Dr. Fox published in 1875, and which was noticed in this Journal (Jan., 1876). Part II. is essentially a condensation of the corresponding sections in his large work, the individual diseases being now simply alphabetically arranged. It is curious to note the number of distinct affections ascribed to " inflammation of the sweat-glands "—*e.g.*, Dysidrosis, Hydroadenitis, Lichen tropicus, Miliaria, and Sudamina. Neither the hairfollicles nor the sebaceous glands can boast of such a pathological retinue as this. To those students who do not care to procure Dr. Fox's well-known large treatise, this small pocket companion will, no doubt, prove very acceptable, and we hope that the next edition will be pruned of such inelegancies as " mal-influences," " semichart," " impurification," " bichloride of hydrargyrum," " baconylike," " cleanish," &c.

Atlas of Skin Diseases. By TILBURY FOX, M.D. London: J. & A. Churchill. 1876. Parts XI. to XVI.

NEARLY all the parts of this useful Atlas have now appeared with commendable regularity, and the very moderate price at which it is issued ought, independently of its merits, to secure it a large circle of subscribers. The plates in Part XI. are good. That representing Purpura simplex and P. hæmorrhagica is very life-like, and it is to be observed that Dr. Fox adheres to Willan's designation of P. urticans, although it is tolerably evident, both from the drawing and from the text, that the condition is really one of urticaria complicated with hæmorrhagic exudation. The Plates in Part XII. are all new, and are devoted to the illustration of Lupus in its chief clinical forms—viz., the erythematous, the non-ulcerating tubercular,

and the ulcerating. In Part XIII. the Plates are likewise all new, and some of the rarer forms of skin-disease are figured. Plate 51 depicts a vesicular eruption which principally attacks the hands and feet, and was described by Dr. T. Fox, in 1873, under the title of " Dysidrosis." We have seen several cases of it within the course of the past year, since our attention was directed to it by Dr. Fox's writings. Parasitic diseases make their first appearance in Part XIV., which delineates ordinary Scabies, and all the varieties of Tinea (ringworm) except Tinea versicolor. The illustrations of Acne in Part XV., and of Molluscum contagiosum in Part XVI. are indifferent. Plate 63 shows a rare form of disease, of which Dr. Fox has seen a few examples, and to which he has tentatively applied the inelegant term of Xanthelasmoidea, from a supposed relationship to the disease (Xanthelasma) represented in the preceding Plate.

Catalogue of the Models of Diseases of the Skin in the Museum of Guy's Hospital. By C. HILTON FAGGE, M.D. London: J. & A. Churchill. 1876. Pp. 269.

THE students of Guy's Hospital possess the enviable good fortune of access to a magnificent collection of models of cutaneous affections—a collection which has been and is still gradually increasing in importance and completeness until now there is, with one or two exceptions, scarcely a disease unrepresented in it, in which the skin is in any way concerned.

Under the modest title of a Catalogue, Dr. Fagge, an accomplished teacher in cutaneous pathology, has really drawn up a laborious and careful description of the illustrative models. This has been revised by Mr. Pye-Smith, and by Mr. Towne, the able artist who, for a period of forty years, has single-handed fashioned both the anatomical and cutaneous models.

A useful glossarial index, drawn up by Mr. Pye-Smith, is appended, and a study of the splendid collection with the aid of this valuable guide-book could not fail to impart a thorough scientific knowledge of a class of affections which, until recent times, has been so generally neglected.

The simple classification adopted is practical and convenient, and a strong vein of good sense runs through the whole work which, from its very nature, does not admit of detailed criticism.

The book contains a large amount of practical information, in the

form of critical and explanatory remarks, not to be found in the ordinary manuals on diseases of the skin, and it deserves a place in every practitioner's library as a trustworthy and helpful work of reference.

Theory of Medical Science. By WILLIAM R. DUNHAM, M.D. Boston: James Campbell. Pp. 150.

THE author of this book believes that a revolution in medical philosophy is inevitable, but unless more tangible weapons than those he has employed are used, it has not much to fear. Dr. Dunham's work might stand as a warning to those who feel tempted to wander from science into transcendentalism. When a writer states that "the supposed power in materia medica and poisons is an error of gigantic magnitude, which has no place in nature, and is the greatest delusion of thought ever adopted by civilised humanity," he places himself beyond the necessity of criticism. His idea of vital force, which alone he holds to be the cause of all bodily states and relations, is a maze of unintelligible utterances. The search for first principles has betrayed him into a production which reads like a parody on metaphysics. We cannot conceive how he persuaded himself that he had grasped either his own or any other theory of medical science.

Alcoholism and the various Forms of Alcoholic Delirium. By DR. V. MAGNAN, Physician to St. Anne's Asylum, Paris; Laureate of the Institute, &c. Translated by DR. GREENFIELD. London: H. K. Lewis. Pp. 258.

CONSIDERING the frequency of the disease in question, this work is certaintly not *de trop.* Probably no one has had greater opportunities of observing the effects of alcoholism than Dr. Magnan, as his experience has been gained not only at the St. Anne Asylum, but also at the Bureau d'Admission, an institution to which no exact parallel exists in these countries. To it are brought all the cases of insanity previous to their admission to the various public asylums. It is here that they are examined, and their admission or rejection decided upon; if admitted, they are drafted to the one or other of the asylums which is most suited to the form of the patient's malady. The first chapter of the book recounts a number of experiments made upon the dog, with a view of deter-

mining the action of alcohol and absinthe. Then the forms of simple alcoholic delirum in man are described, of which it will be sufficient to distinguish three—the maniacal, the melancholic, and the stupid. Dr. Magnan would confine the term *delirium tremens* to the form of the disease which is attended with acute febrile symptoms. He has often seen the temperature rise to 105·8°, and persist at this elevation without any intercurrent affection, which would be in itself capable of exciting or keeping up the fever. In appreciating the temperature it is absolutely necessary to have recourse to the thermometer, as the skin being moist with perspiration may mislead the sense of touch.

In the chronic stage of intoxication a new element comes into play, the condition of the subject is modified; the seed sown is the same, but the soil is different, the alcohol has already made another person of the man. We no longer see merely functional disturbances; a profound action has been produced, nutrition is altered throughout the organs and tissues. Under the influence of alcohol a double morbid process develops itself; the system, as a whole, has been struck with premature old age, and undergoes fatty degeneration, but the fatty degeneration does not stand alone: together with it there occurs a tendency to diffuse chronic irritation, and this double process (sclerosis and steatosis) becomes the characteristic of chronic alcoholism. And according to the predominance of the one or the other of these lesions in the nervous centres, do we see the chronic alcoholic progressing either towards dementia (steatosis and atheroma), or towards general paralysis (diffuse interstitial sclerosis). In the treatment of *delirium tremens*, Dr. Magnan has no very novel views; in the febrile types he disapproves of the employment of chloral as compromising the circulation and respiration. To the translator, Dr. Greenfield, is due a meed of praise. He modestly states, in his preface, that he is conscious of many defects in the translation of the work. His readers, however, will, we believe, discover very few.

Outline Diagram Forms for Clinical Case-Books. Designed by G. ROWELL, M.D.; Resident Surgeon to the Leeds Infirmary. London: Smith, Elder & Co.

THE practice of graphically delineating the site and extent of morbid growths or injuries, or of certain physical signs of disease, is by no means so universal as its utility deserves. Such a method

gives an exactitude and clearness to the report of a case, which in a considerable degree enhances its value as a means of clinical instruction and as a subsequent record. We have for some years past been familiar with the plan of drawing in our note-book, by means of a suitable stencil plate, an outline figure of the thorax, abdomen, &c., on which could be sketched in the position, for instance, of an abdominal tumour, or the extent of a pleuritic effusion. Dr. Rowell's diagram forms are of a much wider range of usefulness. They comprise in a neat and convenient form—similar to that of an ordinary note-book—a large variety of diagrams, which will be found most useful in recording graphically both medical and surgical cases. Each page consists of three or more forms presenting the outlines and chief landmarks of various parts of the body. When the necessary illustration is made, the form is easily detached, by means of its perforated margin, and, the reverse side being adhesive, it can be at once fixed in the case-book.

There are a few diagrams of the fundus oculi, and of the vocal chords, &c., as seen in the laryngoscopic mirror; but the majority of the diagrams are more adapted to surgical than to medical practice. Hence we would suggest that, in future, it would be advisable to issue both a medical and a surgical series of these really valuable forms.

WORKS ON HISTOLOGY.

Frey's Compendium of Histology. Translated from the German by GEORGE R. CUTTER, M.D. London: Smith, Elder & Co. Pp. 263. 1876.

Histological Demonstrations. By GEORGE HARLEY, M.D., F.R.S. Edited by GEORGE T. BROWN, M.R.C.V.S. Second edition. London: Longmans, Green & Co. Pp. 274. 1876.

THE former of these works gives us the substance of a course of twenty-four lectures by Professor Frey of Zurich. The course was an elementary one; and thus the work comes to be a concise account of the more prominent facts and principles of microscopic anatomy. It is illustrated by 208 woodcuts, nearly all of which will be recognised by those who are familiar with Professor Frey's larger work on Histology.

Owing to the vast amount of new material added to the store of histological knowledge by recent research, the standard works on

that subject have become very voluminous, and this tends to pre-
clude the possibility of any one obtaining from them a general idea
of the structure of the various tissues, without devoting an amount
of time and labour which many are unable to bestow. Consequently,
a compendium, such as the present, may fairly be expected to supply
a need long felt.

Unfortunately, however, the English translation is not all that
could be wished. The style in which the original is written is
doubtless very abrupt, and such as to present peculiar difficulties
to a translator. Still we think the task ought to have been better
executed. Many paragraphs are scarcely English, while others
that may be grammatically correct must be extremely difficult of
comprehension to anyone not thoroughly acquainted with the sub-
ject. We quote the following as an example of this:—

"The fluid of the living blood, the plasma, constantly passes through
the thin capillary walls into the adjacent tissues. It brings to the latter
the nutrient materials, to the one these; to the second, again, others.
The fluid becomes impregnated, however, with the products of decom-
position of the tissues. The latter are again different."

The first edition of Dr. Harley's work appeared in 1866. It
was intended for the members of both the medical and veterinary
professions, and was based on the course of demonstrations given
by Dr. Harley, in the physiological laboratory of University Col-
lege, London. We cannot congratulate Dr. Harley on the second
edition. The portion of the book devoted to normal histology
is out of place, as much more ample accounts of histological
methods are to be found in works such as those by Rutherford and
Schäfer. Besides this, the entire histology is behindhand—a cell
is defined as being " uniformly constituted of a membrane termed
the cell-wall, so arranged as to form a sac capable of enclosing fluid
and solid contents." Crystals of hæmoglobin are described under
the name of hæmatin, and no account whatever is taken of recent
researches into the structure of even the most elementary tissues.

The pathological histology, to which some twenty-six pages are
devoted, is likewise altogether behind the age in every way. While
retaining the term "cancer" as applicable to a "malignant growth,"
the author states that " scirrhus, therefore, according to the present
advanced state of our scientific knowledge, can no longer be spoken
of as cancer."

The last forty pages of the book are devoted to parasites—
animal and vegetable—of which there are numerous good woodcuts.

PART III.

HALF-YEARLY REPORTS.

REPORT ON
MATERIA MEDICA AND THERAPEUTICS.[*]

By WALTER G. SMITH, M.D., Dublin; F.K.Q.C.P.I.; Assistant-
Physician to the Adelaide Hospital; Examiner in Materia
Medica and Botany, Pharmaceutical Society, Ireland.

ART. 3. Albuminuria from external use of Iodine.
,, 9. Aloin.
,, 8. Chloroform.
,, 5. Eruptions—(*a.*) from quinine; (*b.*) from salicylic acid.
,, 7. Extract of Malt.
,, 4. Glyceroles—(*a.*) of lead; (*b.*) of bismuth.
,, 2. Leeches, to make them take.
,, 1. Poisonous Cosmetics and Hair Dyes.
,, 6. Salicylic Acid, administration of.

1. *Poisonous Cosmetics and Hair Dyes.*—In spite of the frequent
exposures of the dangerous qualities of most of the cosmetics and
"hair-restorers," these articles are still in great demand, and as
serious mistakes in diagnosis and treatment may and do arise in
connexion with them, we reproduce from different quarters fresh
warnings of their hurtful effects.

Some interesting cases, which have fallen under the observation
of Prof. M. Rosenthal, strikingly illustrate how, under cover of
apparently unimportant phenomena, toxic action may smoulder on
for years without being discovered. The poisonous influence of
cosmetics containing lead may be seen from the following case:—

A lady, aged fifty, from Moldavia, consulted a physician in
Vienna, in the year 1867, about headache, pains and great weak-

ness in the limbs, inconstant painfulness of the ovarian regions
(then looked upon as neuralgia of the ovaries), and also of extreme
nervous irritability. The ailment was declared to be hysteria, and
the patient sent to a hydropathic establishment near the city, where
she remained ten weeks with considerable benefit. In the following
year the patient again came to Vienna, but seriously ill this time.
Dr. Rosenthal was called in consultation, and, being the first to
arrive, was shown into a room abutting on patient's bedroom.
While waiting for his colleague's arrival he had the opportunity of
observing the patient in the next room getting herself up for their
reception. There she was rubbing a white salve into her face,
neck, and arms. A careful examination of the patient showed
great emaciation, striking loss of intelligence, difficult, slow speech,
the movement of the hands, particularly the extension of the carpus,
impeded and accompanied by trembling, the skin of the arms con-
siderably anæsthetic and analgetic; besides, patient complained of
very bad constipation, of pains in the joints, and sleeplessness.
Dr. Rosenthal gave, as his opinion, that the case was one of lead
poisoning, probably arising from the use of a cosmetic containing
lead. Professor Duchek, who subsequently arrived, quite con-
curred in this view. The subsequent examinations of the patient
showed considerable diminution in the electro-motor contractility
of the paretic extensor muscles, and the chemical analysis of the
powder mixed with the fat, constituting together the cosmetic
salve, showed the presence of white lead and carbonate of lime.
The lady slightly improved under the use of the warm sulphurous
springs of Baden, near Vienna, but, on her homeward journey, she
died suddenly with cerebral symptoms. No *post mortem* was made.

In a case of Neftel's, looked upon as hysteria, there were vaginis-
mus and paralysis of the extensor muscles following the use of a
cosmetic that contained lead. With the successful treatment of
the lead affection the vaginismus likewise disappeared. Pauer,
Krimer, and Fievée have also observed nervous affections, cutaneous
anæsthesia, dyspepsia, and paralysis appear after the use of salves
and cosmetics containing lead. Quite lately E. Remak communi-
cated two cases in which cosmetics containing lead produced very
severe cerebral symptoms and paralyses.

The use of mercurial cosmetics is still more dangerous, owing to
the more rapid action of mercury; and in Servia, in the Danubian
Principalities, and with the Roumanian population of Transylvania,
mercurial nostrums are very common and popular articles on the

toilet-table. The following two cases from these districts came under Professor Rosenthal's notice, and show the initial and also the more advanced stages of the affection:—

Case 1 is that of a Moldavian lady, thirty-five years of age, whose face is smoothly painted and smeared. She complains of headache, undefined pains in the limbs, fluttering of the heart, extreme nervous restlessness, and lassitude. Dr. Rosenthal at the time looked upon it as hysterical nervousness, and recommended the use of a chalybeate bath. It was only subsequently that he learned that the patient made use of the so-called *Pasta Pompadour* as cosmetic; and this is composed of white precipitate, nitrate of bismuth, and lard.

Case 2 was a Wallachian lady from Transylvania, forty years of age. She complained of headache, vertigo, arthralgia, trembling of the hands, and insomnia. The disagreeable halitus from the mouth caused him to institute an examination of the mouth. He discovered stomatitis, with necrotic destruction of the walls of several alveoli of the left superior maxilla, and a set of false teeth; and a more careful examination revealed that the patient had never had syphilis, but she had made use of a powerful mercurial cosmetic for her face and shoulders during a period of six months. A portion of this cosmetic was forwarded to the Professor; it was an opalescent fluid, smelling of roses, and having a whitish sediment, which turned out to be calomel.

Pressure and friction suffice to introduce pulverulent salts of lead and mercury into the sudoriparous and sebaceous glands; and, after the rubbing in of mercurial ointments, Overbeck found globules of mercury in the subcutaneous connective tissue, as also in the liver and kidneys. These cases, therefore, are of a kind to call the attention of the profession to the true nature of affections of the nerves and muscles where there is a probability, or possibility, of the patients having made use of such cosmetics, in which cases an early recognition of the fact is of great importance.— (*Brit. Journ. of Homœop.*, from *Mitth. d. Wien. Med. Doct. Coll.*, May, 1876.)

Many of the hair-restorers, or more properly hair-dyes, so extensively advertised and used, both in England and America, afford flagrant examples of the dangers alluded to; and in *The Lancet*, Jan. 13, 1877, are recorded the results of the analysis of twenty-one samples of the best known preparations of this kind now in use in England.

Out of the twenty-one samples examined, in reference to their essential ingredients, no less than fourteen were practically identical in their nature. They contained sulphur in suspension, and also lead in varying, but always in very considerable, quantity, and yet were usually described as "perfectly harmless," "free from injurious substances," and so on. Two of the specimens contained a lead salt dissolved in excess of sodium hyposulphite, and the solution so obtained *does not give a precipitate with iodide of potassium*. This is noteworthy, because, in the handbill which accompanies one of the samples, purchasers are warned against the dangerous hair preparations which contain lead as likely to induce paralysis of the brain and insanity, and are directed to test all preparations with *iodide of potassium*. In one American sample no free or loosely-combined sulphur was found, but only lead in considerable quantity. Another of the preparations was contained in two bottles, in one of which ammonio-nitrate of silver, and in the other pyrogallic acid, was detected. The remaining three preparations analysed were intended for lightening the colour of the hair, and each was found to contain a tolerably concentrated and slightly acidulated solution of peroxide of hydrogen. Thus, out of the twenty-one samples examined, lead was present in seventeen, and, in most cases, in large and deleterious quantity—*e.g.*, 10 grs. of crystallised sugar of lead in ʒi. fl.

2. *Leeches, to make them take.*—Dr. Correa de Serra recommends the following simple method, which long experience enables him to declare is infallible. The part is first well washed with pure water, dried, and then rubbed gently so as to warm the skin. Whatever be the number of leeches (say not more than twenty), we next take a small glass, and lay across its mouth a moderate sized piece of linen. Pushing in the centre of this to the bottom of the glass, a cavity is formed, into which the leeches are thrown. The glass having been applied to the desired spot, and lightly pressed down by the left hand, we tighten with the right hand the linen by drawing it round the circumference of the glass. The leeches are thus necessarily brought into contact with the skin and cannot wander about. Presently the leeches will bite, as testified by the patient, when the glass may be removed, *leaving the linen* for a little longer.—(*Rev. de Thér. Méd. Chir.*, No. 18, 1876.)

3. *Albuminuria produced by topical use of Tincture of Iodine.*— M. J. Simon, while experimenting for several months with a new

treatment for tinea, which consisted in applying to the scalp a mixture of equal parts of glycerin and tincture of iodine, found that, after some time, symptoms of iodism developed themselves in a young girl. He found, moreover, that her urine included iodine and a notable quantity of albumen. Similar researches, made on other children submitted to the same treatment, gave similar results. The urine of each of the children contained iodine, and that of some of them albumen. M. Simon conducted similar experiments on children not affected with tinea, and on whom tincture of iodine was applied to different parts of the body. Examination of the urine, on the day following, indicated the presence of iodine and of albumen, which varied according to the applications of the iodine. M. Simon concludes that, in children, the external use of tincture of iodine always causes the appearance of iodine in the urine, and, in many cases, also of albumen.—(*Journ. de Méd. et de Chir. Prat.*, Mai, 1876.)

4. *Glyceroles—(a.) of Subacetate of Lead.*—Mr. Balmanno Squire is a firm believer in the efficacy of the local treatment of certain chronic cutaneous affections, especially chronic eczema. He has also satisfied himself that lead is an infinitely more suitable agent in the topical treatment of eczema than zinc, and he advocates a solution of subacetate of lead in glycerin as preferable to any other mode of using lead. Glycerin is readily miscible with the eczematous secretion, and keeps the surface of the skin moist and supple. Mr. Squire gives the following directions for the preparation of the remedy:—Take of acetate of lead, 5; litharge, 3½; glycerin, 20. Heat for half an hour in a boiling glycerin bath, constantly stirring, and filter in a gas-oven or other kind of heated compartment. The result is a perfectly clear and colourless liquid, of a somewhat more viscid consistence than pure glycerin. This solution is too strong for general use, although it may occasionally, in certain cases, be employed undiluted. Half a drachm of the above solution to one ounce of pure glycerin is about the strength which is usually expedient; but the proportions must, of course, vary with individual cases. In Mr. Squire's opinion, the glycerole of subacetate of lead is a far more generally useful application for chronic oozing eczema than either the benzoated oxide of zinc ointment, the calamine and oxide of zinc lotion, or the unguentum diachyli. As a first result of this application, the eczematous skin will sometimes appear to be redder and more inflamed, as well as

moister; but this is simply due to the infiltration of the cuticle, and of the scales or thin scabs, whereby they are rendered more translucent. Previous to each application of the glycerole, the skin ought to be well sponged with warm soap and water.—(Reprint from *Med. Times and Gaz.*, March 18th and 25th, 1876.)

(*b.*) *Of Nitrate of Bismuth.*—The value of bismuth as a local application in many skin-diseases is well known, but its use has always been crippled by the difficulty, indeed impossibility, of obtaining a suitable solution. Mr. Squire states that (neutral) nitrate of bismuth is freely soluble in glycerin, without decomposition, and that, even on free dilution of the glycerole with cold water, the precipitation of basic salt of bismuth is greatly delayed, and apparently prevented in great part. This glycerole is a bland and mild astringent, and offers facilities as an application to the throat, larynx, vagina, uterus, and urethra, as well as to the skin. It might also be used internally.—(*Pharm. Journ.*, November 11th, 1876.)

5. *Eruptions—*(*a.*) *from Sulphate of Quinine.*—In an article on this subject in the *Journ. de Thér.*, No. 8, 1876, M. Dumas reproduces the facts noted by Jean de Grissac, in his thesis on quinic eruptions, and adds a case in which sulphate of quinia produced different forms of cutaneous eruption and other singular effects. A lady, affected with facial neuralgia, after having taken only 4·5 grs. of sulphate of quinine, was seized with a fit of asthma, coryza, and fever, attended with violent itching over the whole body, and accompanied by urticaria and by a peculiar eruption, scarlatiniform in some places, and in others distinctly papular. On four different occasions the patient took sulphate of quinine, and experienced similar results. The author also mentions the case of a lady who, after each administration of sulphate of quinine, suffered from an urticarious eruption (*Journ. des Connaiss. Méd.*, No. 18, 1876). In the *Reports* for February and August, 1870, and February and August, 1871, seven cases were adduced in which an erythematous rash, succeeded in six out of the seven cases by desquamation, followed the administration of *small* doses of quinine.

(*b.*) *From Salicylic Acid.*—At the annual meeting of the Clinical Society, Dr. Cavafy read a paper on a "Case of Acute Rheumatism treated by Salicylate of Soda," with intercurrent scarlatina, and a peculiar skin eruption. The patient, a potman, aged thirty-three years, was admitted June 1st, labouring under his second attack of

acute rheumatism, and was put on 3ss. doses of salicylate of soda, at first every two hours, then every four hours, until the temperature was normal, June 5th (9th day). Two days afterwards, febrile symptoms again set in; temperature 100·5°; the throat became inflamed, and next day a punctate rash, precisely like that of scarlet fever, was seen on the neck and thorax. He complained of great itching and irritation of the arms and legs, which were covered with raised erythematous patches, resembling urticaria, but not evanescent. On June 9th pain re-appeared in the right shoulder; on the 11th the sore throat and rash had subsided. The erythematous patches became covered with vesicles, some very minute, others large and confluent, here and there resembling herpes, filled with opalescent fluid. The vesicles afterwards dried up. The rheumatic relapse became more severe, and 3ss. doses of sodium salicylate were again ordered every two hours. Under this treatment a rapid diminution of the rheumatic symptoms took place, but the eruption re-appeared, now extending to the trunk, the vesicles being much less developed. The treatment was persisted in, and the eruption subsided. On June 23rd, the medicine having been discontinued on the 19th, another rheumatic attack occurred, which again yielded to similar treatment—no eruption now following its use, and no relapse occurring. Dr. Cavafy concluded that the eruption was evidently caused by the salicylate of soda; tolerance of the drug being established after a time. He remarked that a similar eruption had been observed by Mr. Callender to result from the external application of the acid (*Trans. Clin. Soc..* IX., p. 9.) In the discussion which followed, Sir W. Jenner mentioned a case of severe burn which was dressed with lint soaked in salicylic acid, in which the application was followed by a general copious eruption, which it was thought might be scarlet fever. This result has not been noticed in Mr. Lister's experience (*Lancet,* Jan. 20th, 1877.) In connexion with this point may be noticed the experience of Prof. Abelin, of Stockholm, on the irritant effects of large doses (12–15 grs.) in young children, albuminuria having been frequently observed, apparently induced by the drug (*Med. Times and Gaz.,* Jan. 13th, 1877). Compare, also, Dr. Brakenridge's observations on the frequency of albuminuria in cases of scarlatina treated with pure salicylate of sodium.—(*Med. Times and Gaz.,* Dec. 2nd, 1876.)

6. *Salicylic acid, administration of.*—Mr. J. W. White believes that, except in small doses, salicylic acid is best prescribed in

suspension with tragacanth, or in the form of pill, especially as there is little doubt that, in soluble combinations with alkalies and alkaline salts, the properties of the free acid are more or less modified. If salicylic acid be beaten up with $\frac{1}{10}$th its weight of borax, and the same proportion of glycerin and tragacanth, an excellent pill mass is procured, of which 6 grains represent 5 of acid, and do not form an inconveniently large pill.—(*Pharm. Journ.* Dec. 16, 1876.)

Mr. J. C. Thresh, of Buxton, undertook some experiments to ascertain whether or not the increased solubility of salicylic acid in solutions of borax and other salts was due to some chemical decomposition between the acid and the salts employed, and if the solutions thus formed possessed the antiseptic and anti-fermentative properties of the free acid. The results he obtained, although not completely worked out, are of interest, and were as follows (*Pharm. Journ.*, Nov. 25, 1876):—

Borax.—This salt is remarkable for its solvent action upon a large number of organic compounds, the nature of which action is not, in most cases, yet ascertained. If borax and salicylic acid be mixed in a mortar, the result is a damp, almost pasty mass. The taste at first is simply that of the acid and borax, but in a very short time it begins to acquire a bitter taste, and after a few hours it will be found to be intensely bitter. If a little of the freshly prepared mixture be carefully fused, the resulting mass at once becomes exceedingly bitter, and if the proportions employed were one of borax to two of acid, the mass is soluble in about twice its weight of water. A dilute solution of 5 grains each of acid and borax, in 1 ounce of water, is devoid of bitterness, and remains so even after keeping a length of time, but stronger solutions soon become bitter. I have failed as yet to ascertain the nature of this reaction, or to isolate the bitter product, unless a crystalline deposit, which is slowly forming in a solution of 2·5 borax, 4 acid, and 50 water, which is evaporating spontaneously, proves to be the substance in question.

Phosphate of Soda.—This salt has not a solvent effect equal to that of either borax or ammonium citrate. One part of salicylic acid requires—

2 parts of phosphate to form a solution with 50 parts water.
2·25   „   „   „   25   „
2·5*   „   „   „   12·5   „

* 3 drams would contain a full dose (15 grains nearly) of salicylic acid.

· Solutions 1 and 2 are colourless, but the strongest solution has a slight pink tint (characteristic of salicylic salts). Diluted with water, ferric chloride added in excess gives a purple red solution, which also indicates the existence of a salicylic salt, since whilst free salicylic acid strikes a purple colour with ferric chloride, its salts give a deep red coloration with this reagent. No phosphoric acid, however, is liberated, for a single drop of the dilute acid, added to the solution, causes a precipitation of salicylic acid.

Ammonium Citrate.—I first ascertained by experiment that this citrate, whilst increasing the solubility of salicylic acid in water to a much greater extent than sodium citrate, yet possesses no advantages over potassium citrate, and as this latter was more convenient for my purpose, I have employed it in preference.

Table of solubility of Salicylic Acid in Potassium Citrate solution.

Sal. Acid	1	Citrate	·75	Water	100
„	1	„	1·0	„	50
„	1	„	1·15	„	25
„	1	„	1·25	„	20
„	1	„	1·4	„	12·5
„	1	„	1·5	„	7·5

A stronger solution than the last solidifies upon cooling, but the nature of the mass I have not yet ascertained. It gives reactions indicative of free and combined salicylic acid, and of combined citric acid, but not of free citric acid. An alcoholic solution of potassium salicylate, mixed with a similar solution of citric acid, gives a precipitate of potassium citrate, which readily dissolves on the addition of a little water, and the solution thus formed is miscible with water, without precipitation of salicylic acid. 1 dr. acid. salicylic, 3½ drs. sp. vin. rect., 1 dr. pot. cit., and 3½ drs. water, form a solution miscible with water in all proportions, and 2 drams of which contain 15 grains of the acid. In this solution diluted acetic acid gives no precipitate, citric acid causes a precipitate to form slowly, mineral acids throw down the salicylic acid instantly. Ferric chloride colours the fluid purple red.

To ascertain the antiseptic value of the solutions formed by aid of these salts, I added them to a number of infusions (malt, quassia, calumba, &c.), to grape juice, and flour paste; and so far as I can tell, after a lapse of two months, with the exception of flour paste and grape juice, the solutions are equally as fresh as those prepared with free salicylic acid.

To test their anti-fermentative powers I prepared over thirty mixtures of flour (1 oz.) and water ($\frac{1}{4}$ oz.), with 20 grains of German yeast in each, and added thereto various proportions of free salicylic acid, of potassium salicylate acidified with acetic acid, and of salicylic acid dissolved by aid of borax, phosphate of soda, and citrate of potash, and in the cases where no fermentation ensued I confirmed the result by repetitions of the experiments.

The smallest quantity of free salicylic acid which uniformly prevented the rising of the dough was 1 grain. The acidified salicylate of potash had not the slightest effect unless added in large proportions. 1 grain of acid in borax solution was equally as powerful as the free acid. A similar quantity dissolved by aid of ammonium citrate or sodium phosphate only retarded for a variable time the fermentation, but in both cases $1\frac{1}{4}$ grains were found effectually to arrest it.

It is, therefore, evident that some reaction, as yet undetermined, does take place between the salicylic acid and the salts employed as its solvents, yet that in whatever state the salicylic acid exists in the above named solutions, it is capable of exhibiting in a high degree all those properties which have conferred upon it such notoriety.

7. *Extract of Malt.*—This preparation has become a popular dietetic remedy, and is particularly esteemed as a demulcent and nutritive food for children. Its syrupy appearance, however, offers many inducements to fraud. The simplest and cheapest adulterant is glucose (syrup), which is in general use by brewers to increase the amount of extractive matter in beer. But there is no ready method known to detect this admixture. And as a complete analysis is in most cases impracticable, the consumer must generally rely upon the honesty of the manufacturer. Hager considers the examination of the following points sufficient to decide on the genuineness and qualities of a malt extract :—

1. The extract must have its own peculiar sweet taste and the refreshing odour of fresh bread.

2. The watery solution must be nearly clear. On dissolving 5 gms. of the extract in 45 gms. of distilled water, by the aid of stirring and without heat, a slightly cloudy solution is obtained, which may be filtered without difficulty. The insoluble matters were found to be different under different circumstances, and consisted of amorphous coagulum, ferment-bodies, and columnar, four or six-sided (sometimes also star-shaped) crystals.

3. 10 c.c. of the filtered solution, prepared as just stated, are placed in a test-tube, 1·5 cm. ($=\frac{5}{16}$ inch) wide, and mixed with 10 c.c. of an aqueous cold saturated solution of picric acid. In the case of good extracts, a strong cloudiness appears at once, which gradually increases, and after ten minutes has become so intense as to prevent the passage of daylight through the liquid.

4. Another portion of the filtered 10 per cent. solution is mixed with tincture of galls in excess, and well shaken. A copious whitish precipitate, remaining suspended in the liquid, and making it impervious to light, must make its appearance.

The same relationship which exists between pepsin and fibrin, or other animal proteïn-compounds, holds good between the diastase of extract of malt and vegetable starches. The latter, which form a main constituent of our vegetable diet, are converted by diastase into dextrin. Extract of malt, therefore, owing to its proteïds and to diastase, is an excellent adjunct in the nutrition of infants.

Various other remedies have been combined with the extract of malt, to modify its action, or it is used as a pleasant disguise for disagreeable medicines. But since those agents which are capable of arresting or preventing fermentation would exert the same influence upon the diastase, and consequently would prevent the latter from acting upon starch, they should not be given in combination with malt extract, or at least only in very small quantities. Tannic acid, salts of quinine, salts of iron (ferric) with organic acids, and potassium iodide, should be given in comparatively large quantities of the extract. Hager mentions the following compounds or preparations as in use in Germany :—

Extractum malti chininatum (or *quinatum*), *Malt extract with quinia*, was formerly prepared by adding 1 part of quinia sulphate to 250 parts of the extract ; but the bitterness of the mixture caused it to be frequently rejected by children. At present the usual method is to add 1 part of quinia tannate to 100 parts of the extract. A trial with a perfectly neutral extract, prepared by J. D. Riedel, yielded a solution which had not deposited any sediment after eight days, and which exerted but a very slightly diminished action upon starch. Hager proposed to call this *Extractum malti tannochinatum.*

Extractum malti ferratum, *Ferrated malt extract.* A formula for this preparation is given by the German Pharmacopœia. It is best prepared by dissolving 2 parts of soluble ferric pyrophosphate in five parts of pure glycerin, and adding it to 93 parts of the extract.

The taste of the resulting product is, however, slightly modified, and Hager recommends to use saccharate of iron 3 parts, glycerin 7 parts, and extract 90 parts. This would be *Extractum malti saccharoferratum.*

Extractum malti iodatum, Iodised malt extract, is a solution of 1 part of potassium iodide in 10,000 parts [rather dilute! Ed. N. R.] of extract.

Extractum malti pepsinatum, Malt extract with pepsin, is said to be more nutritious than the simple extract, and to be especially valuable in dyspeptic complaints. For this purpose a saccharated pepsin of 50 per cent. is recommended. Two parts of this are rubbed with 5 parts of glycerin, and added to 93 parts of the extract. It is best to prepare this mixture only when wanted.

Extractum malti lupulinatum, Extract of malt with hops, is a preparation made by J. D. Riedel, of Berlin. Although originally intended to be added to weak malt liquors or beers for the purpose of giving " body," it may be used medicinally. It has an agreeable aromatic taste, and is probably a solution of alcoholic extract of hops in extract of malt.—(*Pharm. Journ.*, Oct. 14, 1876, from *New Remedies.*)

8. *Chloroform* (*a.*), *internal administration of.*—The following simple method is recommended:—The prescribed amount of chloroform is added to 100 or 120 parts of milk, pure or sweetened, and aromatised with laurel water, and the mixture briskly shaken. The chloroform becomes finely divided, and remains indefinitely suspended in the milk.—(*Rev. de Thér. Méd. Chir.*, No. 21, 1876, from *Union Méd.*)

[This hint is worthy of adoption, and the mixture is by no means unpalatable. 20 m. of chloroform with ʒi. of milk make an excellent emulsion, and although, after some hours, part of the chloroform subsides, agitation at once restores the emulsive condition.—*Rep.*]

(*b.*) *Deep injections of chloroform in obstinate sciatica.*—In 1875, Dr. Collins announced (*Schmidt's Jahrb.*) that he had obtained rapid and permanent cures of old and rebellious cases of sciatica by injecting 30 to 50 drops of chloroform deeply into the tissues of the thigh or nates. Dr. Bartholow repeated this method, and a French physician, anxious to control Collins's assertions, put Dr. Collins's plan into practice, in the spring of 1876, with the best results. He selected old cases of true sciatica, most of which had been already treated with blisters, iodine, actual cautery, &c. The injections

were introduced over the track of the sciatic nerve, in the thigh, or in the calf of the leg, when the pain, as frequently occurs, was more pronounced in the peroneal nerve, and cures as rapid as those reported by Collins were obtained. In other cases success was only relative—*i.e.*, the pain re-appeared after the lapse of a few days, and it is well to be on one's guard against two consequences which are liable to come on. On two occasions complete anæsthesia of the injected leg, lasting for two days, was observed, and passed off as it came—suddenly. No particular pain was felt at the time of the operation, although it is probable that the injection penetrated the sheath of the nerve. In another case, after an injection into the upper part of the calf, a very painful puffiness was developed, which threatened to suppurate, but yielded, however, to poulticing and mercurial frictions. Apart from these cases, no unfavourable circumstance was noted, local or general. The quantity of chloroform injected each time was usually about 50 drops.—(*Rev. de Thér. Méd. Chir.*, No. 17, 1876, from *Bull. de la Soc. Méd. de la Suisse.*)

9. *Aloin.*—In the *B. P.* two kinds only of aloes are recognised, the one obtained from Socotra, the other from Barbadoes; but, since 1869, a considerable quantity of a new variety of aloes has been imported into this country from Natal. The plant from which it is prepared has not yet been identified. Each kind of aloes contains a crystalline principle peculiar to itself, and the three varieties are named Socaloin (or zanaloin), Barbaloin, and Nataloin. Dr. Tilden has shown, and the observation is confirmed by Dr. Schmidt of Halle, that the three aloins are isomeric ($C_{16} H_{18} O_7$), but combine with different proportions of water of crystallisation. At Dr. Tilden's request, Mr. Nelson Dobson undertook an inquiry into the therapeutic value of these different crystalline principles, and tested them in 50 cases in the Bristol General Hospital.

- An easy test by which the three substances are detected and distinguished from each other is as follows :—Cold nitric acid dropped upon socaloin produces very little change of colour; with barbaloin an intense, but quickly fading, crimson coloration is produced; and with nataloin a blood-red coloration, which does not fade unless heat be applied.

The crystalline principle of Barbadoes aloes has had numerous trials as to its efficacy, and especially as regards its purgative action; and the most widely different results have been obtained by various observers. We shall not attempt to account for this differ-

ence of opinion, but content ourselves with the record of our own conclusions based upon the observations we have made in 50 cases. But that this question is not already finally and satisfactorily settled would appear from the following extract taken from one of the most recent works on therapeutics (Wood's):—" Aloin was formerly believed to be the active principle of aloes, but at present evidence is decidedly against this view. According to Messrs. .T. and H. Smith, in doses of one or two grains it is almost a drastic purge; on the other hand, Robinquet took fifteen grains of it without effect, and affirms that it is a simple bitter." Dr. Craig, in *The Edinburgh Medical Journal*, says aloin is the only active principle found in aloes, and he says further that its " uniformity of strength, the smallness of its dose, and the certainty of its action, should commend it to the favourable consideration of all medical practitioners." So far as our observations go, they do not fully agree with the experience of Robinquet or of Dr. Craig. We have generally found that when either barbaloin, zanaloin, or nataloin has been administered in doses of two grains or upwards, there has been, in the majority of cases, some action of the bowels; but most certainly we have never once found doses of one or two grains to be " almost a drastic purge," as asserted by Messrs. Smith.

The before-named crystalline principles were prepared by Dr. Tilden, who assumes the responsibility of their chemical purity. The dose was given in the form of a pill, made by mixing the crystals with conserve of roses (except in twelve cases, in which barbaloin was mixed with hard soap). The observations made had reference only to the aperient action, or otherwise, of the substances under consideration. The persons to whom they were administered were, in most cases, adult males.

The general conclusions at which we arrived were that neither zanaloin, nataloin, or barbaloin were altogether inert, but that each of them, when given in doses of two grains or upwards, produced more or less action on the bowels. Barbaloin in doses under two grains frequently produced some slight laxative action; but with none of the substances, even when large doses were given, did the effect produced approach anything like a drastic purge. Barbaloin appeared to be more active than either of the other two. In our observations we did not give more than two-grain doses of this substance. Between the zanaloin and the nataloin there was not much difference in purgative action; the former was frequently given in doses of four grains without producing more than two

slight motions. Nataloin in similar doses sometimes produced two, three, and even four motions; and sometimes there was no action following the dose.

To summarise the general result of our observations:—It appears that barbaloin, zanaloin, and nataloin, in doses of two grains, are each of them more or less aperient; barbaloin, apparently, rather more active in combination with hard soap than with conserve of roses. They are each of them decidedly uncertain and variable in their action. In those cases in which we have a record, the time which elapsed after the administration before there was any perceptible action varied from two and a half to fifteen hours. It did not appear to us that the crystalline principle was more potent than a similar dose of aloes; nor did we discover any advantages it possesses over an equal dose of aloes—except, perhaps, that griping was rather less common than when aloes alone was given.—(*Pharm. Journ.*, Aug. 19, 1876.)

PURPURA HÆMORRHAGICA.

Two cases of this rare disease, the morbus maculosus of Werlhof, occuring in infants under one year, are described by J. Appenrodt (*Deut. Med. Wochenschr.*, 1876, No. 39). One of these cases terminated fatally. The maculæ appeared and disappeared very suddenly, and they were seen on the face, which usually is unaffected. The fatal case presented all the appearances of being one of septicæmia. The first symptom was epistaxis and the discharge of bloody phlegm. The eruption lasted five days, and the temperature rose on the last day of life to 104° F. *Post mortem* examination revealed great swelling of the mesenteric glands, tumefaction of the solitary follicles, and marked infiltration of Peyer's patches. The spleen and liver were free of effusions of blood, but the kidneys were thickly covered with numerous hæmorrhages. In neither instance was there any error of diet or imperfect sanitary arrangements.

J. M. F.

PART IV.

MEDICAL MISCELLANY.

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Reports, Transactions, and Scientific Intelligence.

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TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

SAMUEL GORDON, M.B., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, February 7, 1877.

DR. HAYDEN, Vice-President, in the Chair.

A Case resembling one of Addison's Disease. By GEORGE F. DUFFEY, M.D., F.K.Q.C.P.I.; Physician to Mercer's Hospital, &c.

I DESIRE to bring under the notice of the Society the case of a man who has, with short intervals, been under my observation for the last six months. The patient was sent up to my ward one morning from the dispensary attached to Mercer's Hospital, having considerable œdema of both lower extremities and of the penis and scrotum. On looking at him casually, I was at once struck with a peculiar dark brownish discoloration of his lower lip; and remembering that Niemeyer* had expressed his opinion that black spots upon the lips and mouth seemed to him to be pathognomonic of Addison's disease, I ventured to say that I believed we had to do with a case of supra-renal melasma. This belief was fortified, on further examination, by finding similar blackish spots on the mucous membrane of the mouth, as well as a marked and extensive discoloration of the skin.

The history he gave at the time was as follows :—He was thirty-two years of age, and single; the eldest of a family of three. A sister died when very young, and his father died of " decline ; " his mother is still alive. For fourteen years he had been a car-driver, and was a hard drinker all the time. Three years ago his licence was taken from him, on account of his intemperate habits, and he has done no regular work since. He never remembers being sick or feeling unwell until the present illness commenced. He could ascribe no cause for it, nor fix any

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* A Text-Book of Practical Medicine. 1871. Vol. II., p. 51.

more precise date for its commencement than that he felt it coming on four or five weeks before his admission; and, for the fortnight immediately preceding this, he had felt himself getting much weaker.

As due partly to his former occupation, but chiefly in consequence of his mode of life, he has received many injuries—*e.g.*, a Pott's fracture, fracture of both clavicles, ditto of left patella, and ditto of nasal bones. An acknowledged worshipper of Bacchus, he has not, he states, been addicted to venery, and never had syphilis. Between four and five weeks prior to admission, he noticed a few little "blebs"—presumably eczematous vesicles—on his right leg. These burst in two days subsequently, discharging a hot watery fluid. The eruption altogether only lasted four or five days. In two or three places near the internal malleolus there are visible on the œdematous leg small superficial scars, such as a bulla would leave. Two or three days after the occurrence of this eruption, he observed, for the first time, that he was getting very dark about the groins; and on looking at himself more closely, noticed that the surface of his trunk generally, but especially at the flanks and in the armpits, had become of a dark tawny-brown colour. He also says that his mother remarked his face was getting very sunburnt. At the same time he noticed the discoloured patch on his lower lip, which he is positively confident did not exist there previously. Naturally he is of a clear complexion, with black hair and grey eyes; and, on admission, the unaffected portions of the integument were of a clear white colour. About a fortnight *after* he had noticed the discoloration of his skin and lip, he felt, for one morning only, a pain in his loins. This he thought might possibly have been due to his having, unknowingly at the time, sprained his back the previous day, carrying some heavy weights. Otherwise he never received any injury to his spine, and there is no tenderness on pressure along it. For five weeks, however, before this he had been feeling listless and weak. He was indisposed to make any exertion; and his spirits, which were usually most buoyant, became greatly depressed. As to the œdema for which he sought relief at the dispensary, he states that he first noticed a swelling of his limbs four evenings previously. The day after the swelling made its appearance it subsided, but returned the following morning, and on the day he came to hospital it was so great that he had to cut his trousers before he could get them on.

State on admission.—He is a stout, thick-set, bloated-looking man, and has a large quantity of subcutaneous adipose tissue; weight 200 lbs.; girth at umbilicus 39 inches; abdominal muscles very tense and rigid. On examination, an almost general discoloration of the face, trunk, and upper extremities is noticed. The face has a dark tan or sunburnt appearance, but the forehead is comparatively free. The labial mucous membrane is pale and irregularly speckled over with well-marked darkish brown spots, and similar patches can be seen on the right buccal mucous

membrane and upon the soft and hard palate. Along the under-surface of the pale and flabby tongue there are some "ink-like streaks,"[a] but none on the gums. The sclerotics are "pearly-white." There is no discoloration of the palpebral mucous membrane, or of the scalp. His hair is black and has not changed colour. On the trunk the discoloration is deepest in hue in the axillæ and on the groins and flanks. In the latter region it is of a dark-brown colour, which can be traced, in a girdle form, round the body, as if it had been mapped out by the belt he habitually wore. Towards the mesian line of the trunk the discoloration gradually faded off into lighter shades of a sepia tint. The discoloured integument is universally and irregularly mottled with small darker spots resembling large freckles. Both areolæ and the nipples are deeply pigmented. The neck is deeply stained; and at the flexures of both elbows and wrists, and on the dorsa and knuckles of both hands, are brownish patches. There is a marked atrophy of the right forearm and hand, and along the ulnar border of the former are two adherent cicatrices, the margins of which are deeply coloured. The nails are normal. The pubes is of a very dark colour, and the extremely œdematous penis and scrotum are also markedly pigmented. The discoloration is also noticed at the inside of both thighs, on the buttocks, and at the flexures of the knee-joints, but not elsewhere on the extremities, except around the site of the eczematous eruption. The lower extremities, especially the dorsa of the feet, are œdematous, and the pallor of their integument contrasts remarkably with the dusky colour of that of the groin and abdomen.

Examining the back, we find the neck, from the roots of the hair, deeply stained. The discoloration extends down the spine to the fourth dorsal vertebra, and from thence to both shoulders—thus presenting a somewhat tippet-shaped appearance. Below this there is about a hand's breadth of nearly normal colour integument at each side of the spine. Proceeding downwards, the cutis becomes gradually of a deeper tint, until at the nates it is as dark as a Mulatto's. In the lumbar region there are a few white and shining linear streaks on the here very dark-coloured integument. Minute white circular islets of skin are, as a rule, universal. The palms of the hands and soles of the feet are not affected. The discoloured integument is dry and smooth, not furfuraceous or itchy; there is no unpleasant smell from it. The varying shades of discoloration in different places undergo no change on pressure; there was no anæsthesia. Temperature in axilla, 97° Fahr. His general appearance at the time did not give one the impression that he was deficient in personal cleanliness. The patient sleeps well but heavily. Has no head-ache, faintness, or giddiness, but is subject to a buzzing in his left ear, and has always been a little deaf in it, and sometimes has a discharge from it. He has a

[a] Med. Gazette. 1861. Vol. II., p. 32; and Greenhow, "On Addison's Disease." 1866. Table K., Case 149.

slight cough with scanty sputa; percussion-resonance is rather deficient posteriorly over the thorax, and the respiration generally harsh, but feeble, and mixed with sibilant rhonchi and small crepitation; any exertion causes breathlessness. Pulse 82, small, feeble, and compressible. No palpitation. Heart sounds, especially the first, feeble, but normal. As regards the digestive system—the tongue is large, pale, and flabby; no loss of appetite, but he is thirsty and complains of his mouth getting constantly dry; no irritability of stomach; bowels regular. Abdomen is tumid, and its wall very tense and rigid, but there is no epigastric pain or tenderness on pressure. No perceptible enlargement of liver or spleen. The urine was scanty, of a lemon-yellow colour, sp. gr. 1,010, and a little frothy on the surface; it contained a large quantity of albumen, as well as numerous large and small epithelial cells, and a few granular tube casts. Heller's test[a] gave the coloration indicative of the presence of indican very distinctly, the reaction being rendered much more marked by the addition of a few drops of nitric acid.

After the patient had been a month under treatment, directed chiefly to the relief of the renal and bronchial symptoms, the anasarca and cough disappeared. One item only in the treatment requires to be particularised— viz., the frequent employment of warm baths; their effect on the discoloration of the skin was *nil,* but they acted well as diaphoretics. The albuminuria persisted, and was a constant symptom during his stay in hospital.

So far the case seemed to be one of associated Bright's and Addison's disease. No one who watched its progress could fail to be struck with the remarkable change in the patient's mental and physical state. On his admission he was comparatively in lively spirits, and would laugh and joke with the other patients in his ward. He had not been long in hospital, however, before a gradually increasing depression of spirits was noticed; he looked stupid and heavy; became very weak, and was quite disinclined to get out of bed and sit up, although urged to do so. He complained of getting short in the wind upon making any movement, even after the mere exertion of raising up in bed to take a drink. The feeble action of the heart has already been referred to. The first sound was especially weak and distant; and the respiratory sounds, after the relief of the bronchitis, of very feeble intensity.

After the disappearance of the œdema, he began to complain of frontal head-ache; this was persistent, lasting for fully a month. It may have been due to the renal mischief, but persistent and obstinate head-ache such as this was, has been noted in a few cases of morbus Addisonii. As a transient symptom of that disease it is common;[b] and severe pains in the

[a] "On the Urine." By Neubauer and Vogel. New Sydenham Society. 1873. P. 46.
[b] Jaccoud. Dict. de Méd. et de Chir. Prat. Tome V., p. 704. Article. Bronzée (Maladie).

head may usher in an occasional rapid termination by the sudden access of cerebral symptoms.[a] It occurred to me that in Addison's disease there might be some abnormal pigmentation of the choroid or retina, and consequently I requested Mr. Swanzy to make an ophthalmoscopic examination of my patient. With the exception of a small posterior staphyloma, neither Mr. Swanzy nor Dr. Fitzgerald—who, at a subsequent period, was also good enough to examine his eyes—discovered anything abnormal. I have subsequently ascertained, from a perusal of Dr. Louis Martineau's Monograph, "De la Maladie d'Addison" (Paris: J. B. Baillière et Fils, 1863, pp. 34, 44), that an ophthalmoscopic examination was made in two of the cases of that disease he has reported. In both there was a large quantity of pigmentary matter accumulated on the choroids, but no deficiency of visual power in either. A drop of my patient's blood, examined microscopically, appeared to contain a larger number of white corpuscles than healthy blood should.

When he had been a month in hospital the labial patches became faint, and ultimately entirely disappeared; while, simultaneously, notwithstanding numerous warm baths, the general cutaneous coloration became darker. On the flanks and loins, where the tint was most marked, the papillæ, as it were, stood out in relief, giving the skin a rough appearance, somewhat resembling *cutis anserina*; and drawing the point of a pencil now quickly over the loins left a white track.

About a fortnight after this he complained of a severe dull pain in the left infra-axillary and hypochondriac regions, which lasted for some time, and for which no cause could be ascertained. He was now given gr. $\frac{1}{40}$th of phosphorus in pill (Kirby) twice daily. His weight at this time had fallen from 200 lbs. to 160 lbs. He complained of feeling cold, was very silent and desponding, would not leave his bed, and said he "had no heart for anything." Soon after his admission he commenced to have frequency of micturition, and often had to get up to pass water five or six times at night. On the 25th of September, the urine of twenty-four hours amounted to 45 oz.; its sp. gr. 1020; it was albuminous, and, tested by Russell and West's method,[b] contained 1·1%/$_o$ of urea; indican seemed also in excess. He complained of pains in the loins, but said he felt a little stronger, and that the pills were keeping him warm. He had a good appetite and no constipation. After he had taken the phosphorus for three weeks, he felt a good deal stronger. He was in better spirits, used to get up and walk about, and was not so easily put out of breath. He had gained 6¾ lbs. in weight, and felt warmer. There was no change as regards the state of the cutaneous discolorations, and although the patches on the lower lip had vanished, there was still a deposit of pigment inside the mouth, on the right side, which reminded one exactly, as

[a] Pye-Smith. Brit. Med. Jour. 1876. Vol. II., p. 740.
[b] Practitioner. Feb., 1875. Pp. 86 and 126.

Trousseau has pointed out,[a] of the appearance observed in the buccal mucous membrane of some dogs. He now went to the Convalescent Home for a fortnight; and on his return it was noted that he looked better and that his colour was clearer. Four days after he was re-admitted with a rather sharp attack of hæmaturia. This was checked by the administration of ergot, and after a stay of three weeks in hospital he left, considerably improved in every respect, but only to be re-admitted again in a few days with a return of the œdema of both lower extremities. The affected portions of the body were now of a somewhat lighter tint, and it was noticed that, corresponding to the site of a blister—which had been applied over the left side, when he had been suffering from pain in the region of the spleen—the cutis was of a much paler colour than the surrounding integument. This is opposed to the usual result following the application of a blister in Addison's disease, increased pigmentation subsequent to vesication being the rule. Soon after his admission, and mainly as a test experiment, a small blister was applied to a portion of the unpigmented skin of his back; no discoloration ensued. These facts, coupled with the partial and mottled, instead of uniform and general, discoloration now observed; the disappearance of the patches first from the lips and now from the mouth; the greatly improved general health, strength and spirits of the man; his good appetite; the absence of emaciation and gastric symptoms, and the perceptible clearing up of the discoloration now following the frequent use of warm alkaline baths and soap caused me to question whether the case was really one of Addison's disease or not. This doubt, however, was, I confess, not dispelled until after a perusal of Dr. Greenhow's case of "Vagabond's Discoloration Simulating the Bronzed Skin of Addison's Disease," published in the last (the ninth) volume of the Transactions of the Clinical Society.

My patient left hospital on the 4th of January. I have frequently seen him since, and this morning made a careful examination of his condition.

His person and clothes were dirty, but his skin is decidedly clearer than when he left hospital. There is still a brownish mottling, resembling ephelides, over his trunk, and the integument is rough; the nipples and their areolæ, and the penis, are still pigmented; the site of the blistered surface on the side is paler than the surrounding integument; there is no discoloration of the lips or cheeks, but a faint depression can be detected in the buccal mucous membrane, corresponding to the former situation of the patch; the tooth (2nd upper molar) in its immediate vicinity is not rugged, but is very deeply stained black, probably from tobacco smoke; the under-surface of the tongue at each side of the raphe presents numerous deep purplish spots, more resembling varicosities than pigmentary patches. His general health appears very good, and he has no urinary trouble, but the urine is still albuminous.

[a] Clin. Méd. Tome II., p. 679.

The diagnosis of Addison's disease is, according to Dr. Greenhow,[a] "founded upon the constitutional symptoms, aided, in a large majority of cases, by the presence of more or less of the peculiar change of colour in the skin." The constitutional symptoms of this disease, according to the same authority, are—progressive asthenia, often originating without any apparent cause, and seldom dating from any definite period; an indescribable aspect of listlessness or depression; great languor and indisposition for exertion; remarkable weakness of the heart's action, and small, feeble, compressible pulse; loss of appetite; irritability of stomach; nausea and retching (p. 9). With the exception of the gastric symptoms, all the others were present in my case. Similar symptoms are, however, not uncommon in some forms of renal disease; so that the probability of their being due to this cause in the present instance must not be lost sight of. The absence of all gastric disturbance at first might, it was thought, be accounted for by the early stage of the disease at which the patient had come under notice, and did not prevent several competent authorities, whom I asked to see the case, agreeing then with me in considering it as one of bronzed skin. Only on one occasion—three weeks subsequent to his admission—was there any sickness of stomach. This occurred one day after the patient's dinner; he vomited several times and complained of abdominal pain and tenesmus.

M. Jaccoud, in his admirable article on the Maladie Bronzée, in the *Nouveau Dictionnaire de Méd. et de Chir. Pratiques* (Tome V., p. 704), gives an analysis of 127 cases of Melanodermie Asthenique (as he has proposed to call Addison's disease), with lesions of the supra-renal capsules. Out of this number, obstinate vomiting was noted in 74 instances. So that while an increasing asthenia and discoloration of the skin are, according to him, constant symptoms of Addison's disease, gastric disturbances, and lumbar abdominal pains are not essential symptoms, but are so frequently present that they may almost be considered as characteristic. Other symptoms, which have been laid stress upon as occurring in Addison's disease, and which I have noted as being present in my case, are—the breathlessness on making any exertion; the absence of emaciation in the early stages; the sub-normal temperature; the sensation of coldness of the extremities; the deficiency of urea;[b] the (apparent) excess of white blood corpuscles; the spasmodic rigidity of the abdominal muscles; and the more or less severe and frequent pains in the head, loins, and hypochondria.

The peculiar change of colour in the skin in Addison's disease "is due to deposit of yellowish-brown pigment in the deeper layers of the epidermis, more especially in the layer in immediate contact with the papillæ." It "is not uniform over all parts of the body;" and, as in my case, is deeper on "those parts which are naturally the seats of more pigment

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[a] On "Addison's Disease." Croonian Lectures. 1875. P. 97.
[b] *Cf.*, however, Practitioner. *Loc. Cit.*, p. 126.

than the general surface—namely, the abdomen, axillæ, groins, genital regions, nipples, and especially the areolæ."[a]   Greenhow says that, in his opinion, the deep discoloration of these last may be regarded as one of the most decisive external signs of Addison's disease; and that the appearance of small well-defined black specks like black moles, or already discoloured portions is, in his experience, another no less certain sign. These characteristics were observed in O'B——'s case, and the transition from the darker to the paler shades of the discoloration on different parts of the body was never abrupt.   Greenhow remarks that when the skin has suffered any superficial abrasion or injury—as, for instance, when the patient has been blistered at some recent period—the injured parts become darker than the surrounding portions of the surface, and present defined margins coinciding with the extent of the cutaneous injury. Cicatrices of deeper injuries, on the contrary, usually remain, and are surrounded by a distinct, though ill-defined, border of more or less dark discoloration, contrasting strongly with the ivory-like hue of the cicatricial tissue.   Such were also the characters of the cicatrices on O'B——'s arm; but the site of a blister applied to the hypochondriac region, instead of being darker than the surrounding parts, is, as I have already mentioned, much lighter, and has a well-defined margin.   Such was also the result in Dr. Greenhow's Case of " Vagabond's Discoloration Simulating the Bronzed Skin in Addison's Disease," which I have already referred to, as well as in his former case of " Discoloration of the Skin Simulating the Bronzed Skin of Morbus Addisonii," published in Vol. XV. of " The Transactions of the Pathological Society of London" (p. 226).   That some slight local irritation of the surface may excite a greater deposit of pigment in certain parts, appeared to have some foundation from the deep discoloration around my patient's loins. Dr. Greenhow and others have seen dark streaks on the skin corresponding with the lines of pressure exerted on particular parts by petticoat strings and garters.   In a case recorded by Mr. Nicholson, it is stated that the patient, a baker's lad, presented on his shoulders dark stripes, corresponding to the bands by which the basket he carried was slung at his back.[b] It is probable, however, that in persons not addicted to cleanliness, an accumulation of dirt would take place in those very regions where in my case it was most noticed—viz., round the loins below the trousers belt, and on the back about the shoulders.   Both of these are just the parts which would escape a superficial and occasional ablution, and a constant friction with dirty clothes might account for the ingraining of the integument.

I have mentioned that my patient had also dark spots or patches upon the mucous membrane of the lips, cheeks, and palate, and upon the tongue.

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[a] Greenhow. *Loc. Cit.*, p. 15.
[b] Greenhow. *Loc. Cit.* p. 18.

We know that similar patches are seen in many dogs' mouths; and Greenhow states that Lascars often present similar stains. In fact, he looks upon the pigmentation in this situation, in such persons, as another point of analogy between the normal colouring of the darker races of men, and abnormal pigmentation of Addison's disease. In the former it is a natural physiological condition; in the latter it is produced by the operation of pathological causes. In two of Dr. Greenhow's cases the stains corresponded with seats of irritation produced by the pressure of rugged protruding teeth. No such source of irritation existed in my case, nor would a rugged tooth be likely to excite irritation of the prolabium or of the tongue in an adult. Following Niemeyer, I looked, at first, upon these discolorations of the mucous membranes as pathognomonic of Addison's disease, and was greatly surprised to see them eventually disappear. Alternations in the shades of the cutaneous discoloration, corresponding to improvements and exacerbations in the usually paroxysmal mode of progress of the disease, have been described; but there is no mention, that I can find, of similar remissions in the pigmentation patches on mucous membranes. This it was which first led me to doubt whether, in addition to the indubitable renal disease, there was any adrenal complication whatever; and whether the constitutional symptoms were not due to the former, and the discoloration of the skin a distinct condition.

It is well known that discoloration of the skin may exist without any disease of the supra-renal capsules, and the converse of this proposition holds equally good. Discoloration of the skin may be induced by dirt and uncleanly habits, pityriasis versicolor, syphilis, uterine or hepatic disease, phthisis, leucocythæmia, and, especially, by malarious poisoning. In fact, all diseases attended with a rapid cachexia, the paludal fevers amongst others, appear to conduce to a hyper-pigmentation of the skin.[a] Jaccoud, in speaking of the diagnosis of Addison's disease, mentions the discoloration produced by the prolonged internal use of salts of silver, as well as that due to another less known cause—viz., the absorption of aniline preparations. The pigmentation of the skin, caused by lengthened exposure to radiated heat from fires, or to the sun, or that which accompanies long-continued dermatoses, associated more or less with local hyperæmia,[b] may also be alluded to. In this case there was no history of the patient having had either malarial fever or syphilis. The discoloration being neither raised, in patches, itchy, nor scurfy, in no way resembled pityriasis versicolor; there was no detectable hepatic or tubercular disease; and, if not the result of supra-renal melasma, or of the cachexia induced by the renal disease, the change of colour in the skin was most probably an example of that condition to which Vogt has given the name

[a] Lyon Méd. No. 43, p. 6.
[b] W. Frank-Smith. Journal of Cutaneous Med. Sept., 1870.

of " Vagabonden-Krankheit;" and which Greenhow has rendered into English as " Vagabond's Discoloration."

Against this view are—

1. The age of the patient. Vagabond's discoloration is most frequently seen in elderly persons of very indigent circumstances and uncleanly habits, especially when infested by vermin; while Addison's disease very seldom occurs in persons past middle age.[a] 56·75 per cent. of the 37 cases of Addison's disease, tabulated by Dr. Hayden,[b] occurred between the ages of twenty and forty years.

2. The presence, at first, of associated pigmentation of the mucous membrane and cutaneous discoloration.

3. The constitutional symptoms and sub-normal temperature.

4. The non-effect, at first, of hot baths; the pigmentation actually becoming darker during their use.

In favour of Vagabond's discoloration are—

1. The fact of its being paler on the face, hands, and other exposed parts.

2. The partial discoloration of the skin.

3. The surface left after blistering being pale.

4. The remarkable diminution of the discoloration after the use of warm alkaline baths and soap.

5. The constitutional symptoms present being referable to a distinct cause, and one of the most characteristic constitutional symptoms of Addison's disease—viz., obstinate and uncontrollable vomiting—being wanting.

The co-existence of Addison's and renal disease is not unknown, although rare. Reference to the tables given by Greenhow, Hayden, and Jaccoud, will show about ten cases of discoloration of the skin associated with some form of renal disease, chiefly tubercular, or with albuminuria.

The fact that no symptoms obviously referable to other morbid conditions can possibly justify any assumptions as to the existence of Addison's disease, have led me to conclude that this case was, after all, one of Bright's disease and Vagabond's discoloration of the skin; and I confess that the most perplexing part of the case, to my mind, is the co-existence with the discoloration of the skin of the patches on the buccal mucous membrane and lip, and their subsequent disappearance.

In a case of suspected Addison's disease, reported by Dr. Robinson to the Glasgow Pathological Society,[c] the most marked pigmentation was in the mouth. Dr. Robinson considered that the pigmentation of the mouth made up for the defects in the usual deposition in the cutaneous pigmen-

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[a] Greenhow. Croonian Lectures, p. 43 ; and Clin. Soc. Trans. Vol. IX., p. 46.
[b] Dub. Jour. of Med. Sci. Feb., 1865.
[c] British Med. Journal. 1876. Vol. I., p. 115.

tation, and in the absence of all signs of organic disease (query, *other* organic disease) lead him to regard the case—that of a woman, aged sixty—as one of Addison's disease.

I took the liberty of writing to ask Dr. Greenhow whether, among Europeans, and independently of any local irritation, he had ever known such patches of discoloration on the mucous membranes to appear and disappear in the way I have described. Dr. Greenhow has been good enough to inform me that, personally, he has not seen a case in which discoloration of the mucous membrane of the mouth has disappeared, but that he has been told of such a case by a hospital physician of high reputation, who had proposed that the patient should see Dr. Greenhow. In that case the constitutional symptoms of Addison's disease were absent ; and the discoloration having subsided, and the patient being so little ill, he demurred to the consultation.

In Dr. Greenhow's case of Vagabond's discoloration,[a] he notes that the lips presented some faint dark lines, and that there were indications of light-brown mottling on the mucous membrane of the cheeks. In reply to my query as to the probable causation of a similar, but more marked, abnormal pigmentation in my case, supposing it also to be one of Vaga-bond's discoloration, he says—" I could not explain the pigmentation of lips, &c., in the case of Vagabond's discoloration you refer to when I was watching the case, and I am still equally at a loss to do so." Dr. Greenhow also informs me that he has seen marked discoloration of the mucous membrane of the mouth in cases of very chronic phthisis and of splenic leukæmia. He has reported a remarkable case of the kind in phthisis, in Vol. XXIV. of "The Transactions of the Pathological Society of London." Thinking that probably discoloration of the buccal mucous membrane might, if looked for, not be so uncommon, I applied to two of the leading members of our profession in this city practising dentistry, as being persons whose line of practice would most likely lead them to have observed such pigmentation did it commonly exist. Both Mr. Moore and Mr. Baker, however, state that they have never seen any such patches of pigment.

In Dr. Greenhow's case of Vagabond's Discoloration, as in mine, a drop of blood taken from the finger was found, on microscopic examination, to contain a considerable excess of white corpuscles. The real difficulty of diagnosis in both cases, to again quote Dr. Greenhow's words, " lay in the fact that, co-existing with the general discoloration of skin were some constitutional symptoms and local signs which, at first sight, raised the question whether Addison's disease might not also be present."

[Previous to the discussion on the foregoing case, the subject of it was introduced, and examined by the members present.]

[a] Clin. Soc. Trans. *Loc. Cit.*,

The CHAIRMAN said he was sure the paper had elicited the interest of all present. The main question had reference to diagnosis; and he was inclined to agree very much in the view taken by Dr. Duffey, which was now confirmed by their inspection of the patient—viz., that the case was not one of genuine Addison's disease. He had seen the man, with Dr. Duffey, in Mercer's Hospital some months ago, and he certainly then presented very many of the features of the genuine disease. He had then discoloured lips and skin; he was exceedingly low; and less robust than he was at present; and the whole aspect of the case made him suspect that it would turn out to be one of Addison's disease. Dr. Duffey had admirably summarised the general features of the disease, with which many members present were familiar. The constitutional symptoms were such that when they were associated with the discoloration, &c., it was difficult to fail to identify the disease; but when those constitutional symptoms were defective, the recognition of the disease was surrounded with very great difficulty. The family history of phthisis embraced in the narrative of the case in question, and the presence of old strumous scars, were calculated to lead to the suspicion that scrofula was latent in the man's system. He quite agreed with Dr. Greenhow that the effect of a blister or of any superficial irritation of the surface in producing increased coloration on pigmentation was very characteristic. On the other hand, when the skin was destroyed in its totality the result was the opposite, a leucodermatous condition existing. The appearance of the patches on the buccal mucous membrane and lips, and also of the sclerotics, was very characteristic. The way the case began, and the aspect of the facies, in Dr. Duffey's patient, were very striking. In a case that he (Dr. Hayden) had published some years ago, the same symptoms were observable, including the apathy. There was an almost total indifference to what passed about the patient, and his carelessness as to the result of the disease was most remarkable. This was well expressed in the face of the woman, aged thirty-eight, a coloured photograph of which case he now exhibited. In another case, included in Greenhow's tables, the patient was a boy, aged sixteen, and the disease ran a rapid course.

SURGEON-MAJOR JACKSON, C.B., remarked that a relative of his was attacked about a year ago with somewhat similar symptoms to those that occurred in Dr. Duffey's case. He had large dark bronze patches on his shoulders, neck, and abdomen, in the axilla, and around the breast and nipples. He suffered also from general debility. At first the symptoms seemed to depend on inaction of the liver. He also suffered from anorexia, constipation, and difficult digestion. The temperature was subnormal. Dr. Hudson saw the patient and considered the case—although not a confirmed case of Addison's disease—a suspicious one-

Subsequently the bronze spots disappeared, and his skin assumed a much fairer tint than even before his illness. The urine was albuminous, and this condition persisted after an improvement of· the patient's health had taken place. Sometime afterwards his vision became impaired, and Mr. Wilson found that he had hæmorrhagic retinitis. He improved until about a fortnight since, when, after feeling out of sorts for a week, he had an attack of aphasia. The dark coloured patches again appeared on his shoulders. He gradually improved as regards the aphasia, but the albuminous urine has continued during the whole time of his illness. The teeth were black during the coloration of the skin; but when he commenced to gain flesh the skin got more the colour which it has in cases of Bright's disease, and the teeth became quite clean again.

DR. NIXON said the photograph he held in his hand failed to portray the intense amount of pigmentation present in the case it represented, and which was believed to have been one of Addison's disease. The patient had been under his care in the Mater Misericordiæ Hospital. He was a man about twenty-five years of age, a farm labourer. For some three weeks before his admission he had been pitching hay, and he thought he gave his back a strain. He complained of pain and sense of tightness in his stomach; he had also profuse sweating, and shortly afterwards vomiting set in. At first it was not very urgent, but it was extremely so at the time of his admission into the hospital. About eleven months before his admission he noticed that his skin had become discoloured. The discoloration first commenced on the backs of his hands. He was a man of cleanly habits, and was anxious that his hands should be white; he washed them frequently but could not remove the discoloration. In May last his condition in the hospital was very anæmic. There was great pallor of the conjunctiva and of the mucous membrane of the cheeks and lips, except in certain spots which were covered with dark blotches; there was pearly whiteness of the sclerotic, and intense whiteness of the nails. In addition to the vomiting he suffered from pain in the small of his back and in the epigastrium. The sounds of his heart were fairly marked, and there was no murmur. His spleen was normal, and there was no excess of white blood corpuscles. A slight roughness of respiration existed under the left clavicle, and there was some dulness there. For eight or ten days after he came into the hospital there was considerable difficulty in checking the vomiting. After a while the vomiting passed away, the man gained strength, was allowed out, and was soon well enough to return home. He came back in three or four months afterwards. The discoloration had almost entirely disappeared but was marked in certain spots—namely, the back of the neck, the legs, the axillæ, the region extending up from the

hips to the axillæ, between the nates, on the inner side of the thighs, and the genitals. In all other places the discoloration had disappeared. He had no doubt, from the presence of the constitutional symptoms of the so-called supra-renal melasma, that the man had Addison's disease. Dr. Nixon proceeded to refer at length to the pathology of Addison's disease, basing his remarks on the views expressed by Jaccoud in the article referred to by Dr. Duffey. That writer assumed that the disease was the result of an alteration in the great sympathetic nerve-ganglia of the abdomen. A special function of these ganglia was to preside over pigmentation and the general condition of the arterial system. Under certain circumstances these ganglia might be irritated, and, as a consequence, give rise to the different symptoms so common to supra-renal disease. Disease of the supra-renal capsules was not, however, the sole cause of the symptoms which we are familiar with under the name of Addison's disease. Other abdominal lesions may produce the same results, but only by acting on the semilunar ganglia in the same way that disease of the supra-renal capsules does. But from the extemely large number of nerves that lead to the supra-renal capsules, disease of their substance offers a greater means of transmitting irritation to the semilunar ganglia than any other abdominal disease. The probability that there might be an under-current of inflammatory action in the capsule which might extend along the nerves, and ultimately implicate the semilunar ganglia, and which might be subject to periods of remission, would explain what Dr. Duffey had pointed out—namely, that many of the symptoms of Addison's disease were liable to remission. In connexion with that view cases had been recorded in which the disease had demonstrably travelled up along some of the nerves from the supra-renal capsules to the semilunar ganglia, which were found in an abnormal condition.

PROFESSOR MOORE said that his case of bronzed skin,[a] to which allusion had been made, was not simply one of supra-renal melasma. The abdominal glands generally were enlarged, and there was also some tubercular or strumous disease of the lung, but there was no disease of the supra-renal capsule. Dr. Hughes's case[b] exhibited the same kind of general strumous deposit, together with strumous disease of the supra-renal capsule and tubercular deposit in the lungs and elsewhere. Dr. Martin had also a case, one of a young and fair girl who had the characteristic discoloration of the skin, with patches on the tongue and on the mucous membrane. In his (Dr. Moore's) case there were all the symptoms of Addison's disease in its most aggravated form, including dementia and complete regurgitation of everything. The urine was normal; the blood contained a slight increase of the white corpuscles; but the question

[a] Dublin Journ. of Med. Science. February, 1871.   [b] *Ibid.* Nov. 1865.

was, had these white corpuscles increased absolutely or only relatively.
Dr. Bennett and others made a careful microscopical examination of the
supra-renal capsule, but no one who examined them saw any sign of
strumous disease about them; so that, in that case, there were all the
most marked and progressive signs of Addison's disease, without any
tubercular or strumous disease of the supra-renal capsule. But there
was strumous disease elsewhere—namely, in the abdominal glands, and
also tubercular disease of the lungs. It was said by Dr. Habershon and
Dr. Wilks that, in Addison's disease, there was a specific deposit in the
supra-renal capsule, and Habershon explained the vomiting that occurs
as being due to reflex irritation of the pneumogástric nerve. His (Dr.
Moore's) case upset that theory. There was no prominent enlargement
of the supra-renal capsule, and there was no retention of food at all; the
asthenia was also extreme, and, for three weeks before death, the woman
ceased to speak audibly. About eighteen months ago he had under his
care another remarkable case of discoloration, which differed from the
first. It was a case of deep jaundice about the face and head, while
there were two large leucodermatous patches on each cheek, with a point
of jaundice on each. On the neck and arms, and down towards the
lower extremities, there was a mixture of deep jaundice and melasma.
In this case also there was vomiting and great asthenia, and it proved
fatal. The chief *post mortem* change was scirrhous obstruction of the
bile duct. He (Professor Moore) concluded, therefore, that they could
have any amount of cutaneous discoloration, in the absence of enlarge-
ment of the supra-renal capsules, either from strumous or malignant
disease.

SURGEON-MAJOR GORE observed that, in malaria fevers, discoloration
of a brownish yellow character was very common; and where boils
occurred the cicatrices became quite brown, and remained so for a long
time.

DR. FINNY objected to the name, "supra-renal melasma." A disease
should be called after the pathological changes that caused it; but
Dr. Greenhow had recorded in his book nearly twenty cases in which
bronzing of the skin occurred without any pathological changes in the
supra-renal bodies. From the proximity and sympathy between the
supra-renal bodies and the semilunar ganglia, it was quite sufficient to
consider the disease as due to sympathetic alterations, without binding
themselves to include affection of the supra-renal bodies as part of the
cause.

DR. DUFFEY said he was glad he had brought the case forward,
although it was as yet incomplete, as it opened up the whole subject of
abnormal pigmentation, about which, he believed, very little was known.

He wished to ask Surgeon-Major Jackson whether the urine had been examined for bile-pigment in his case, and if the conjunctiva was tinted. It struck him (Dr. Duffey) that the discoloration might be the result of the hepatic affection the patient suffered from, probably resembling what old writers called " black jaundice."

SURGEON-MAJOR JACKSON.—The urine was not examined for bile, but the conjunctiva presented no icteric tint.

DR. DUFFEY observed that Dr. Nixon had correctly stated Jaccoud's theory as to the pathogeny of Addison's disease—namely, that it was due to an irritation which had its point of departure from the supra-renal glands, the centre of radiation being the semilunar ganglia, and the pigmentary hyperæmia being the result of the abnormal development of function. His views were corroborated by the lowering of temperature which had occurred in several cases. Professor Moore's case, with the exception of the extreme blackness of the discoloration, was by no means unique. Jaccoud has collected seventeen cases of discoloration of the skin without any lesions of the supra-renal capsule, and Dr. Greenhow a larger number, twenty-one, not including Professor Moore's case, which, however, he believes to have been of an analogous nature. In Dr. Moore's second case, which was shown to the Pathological Society in April, 1874, the jaundice and associated melasma were evidently due to the obstruction of the biliary duct. In connexion with the darkness of the tint in that case, and the patches of " leucoderma " on the cheeks—which, however, at least, had a jaundiced tint—he (Dr. Duffey) would refer to the cases of xanthelasma or xanthoma reported by Drs. Fagge and Pye-Smith in Vol. XXIV. of the Transactions of the Pathological Society of London. In these, as in other reported cases, there was a peculiar and special form of jaundice, the skin being of an olive brown or black tint, associated with the peculiar change in the skin called xanthelasma.

The Society then adjourned.

<div align="center">FORMULA FOR ERGOTIN.</div>

MR. CHARLES MITCHELL recommends the following, which he states to be in every way satisfactory :—℞. Ergot, in fine powder, ℥viij.; acetic acid, f℥ij.; alcohol, f℥iv. Moisten the ergot with a mixture of the acid and f℥viij. of water. Let it stand twenty-four hours, pack in a percolator, and exhaust with water; evaporate to four fluid ounces, add the alcohol ; let it stand several hours, filter, and evaporate to an extract. Result, about 480 grains. One grain is equal to eight grains of ergot.— *Richmond and Louisville Med. Journal,* Dec., 1876.

<div align="right">K. F.</div>

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PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-NINTH ANNUAL SESSION.

Saturday, January 13th, 1877.

THOMAS DARBY, F.R.C.S.I., President, in the Chair.

DR. ATTHILL.—I have to exhibit two specimens of uterine polypi, both of which were removed recently from patients in the Rotunda Hospital. One is an intra-uterine and the other an extra-uterine polypus—both were removed from unmarried women—the former from a woman sent in suffering from menorrhagia of considerable standing. On examination, the uterus was found to be enlarged, and the cause of the hæmorrhage not being apparent, I proceeded, in accordance with the rule I always follow in such cases, to dilate the uterus. I accordingly introduced five pieces of sea tangle, but, contrary to what is usual, I found in removing them that the os internum was dilated, while the os externum was still so small that I could barely touch the lower edge of what I was satisfied was a polypus, but had not sufficient room to pass up an ecraseur. I therefore thought of introducing one of Barnes' bags with the view of dilating the os fully, but before doing so resolved to attempt the removal of the tumour with the aid of Gooch's canulæ, which some years ago I adapted to an ecraseur, and with them succeeded in carrying a wire round the pedicle, which was slender, and in removing the growth.[a] The result in this case was most satisfactory, for it saved the patient from prolonged suffering and its attendant risk, for I find that the danger of unpleasant consequences following is, in these cases, generally in a direct ratio to the duration of the dilating process. Still, in this case, though so little violence was used, a rather sharp attack of pelvic cellulitis followed. I should add that we have had almost an epidemic of cellulitis in the hospital—two or three of the cases resulting in the formation of abscesses. Some of these cases occurred in women who had been delivered in the lying-in department of the hospital, had left it quite well, and who subsequently, at periods of from two to four weeks after their delivery, were attacked with cellulitis. In one or two cases it followed operations about the uterus. The other polypus, which is a great deal the larger of the two, was removed this morning. As in the last case, the patient was unmarried, but the tumour sprung

[a] See Lectures on Diseases Peculiar to Women. Fourth Edition. Lecture VII.

from the cervix, and projected into the vagina, and was removed with the greatest ease. In this case menstruation had continued normal. The only thing the woman complained of was profuse leucorrhœa.

The PRESIDENT.—With respect to the tendency to cellulitis, I have in my hospital a woman who was confined without any peculiar difficulty, and after four or five weeks she got a rigor and feverish symptoms, ending in what I believe to be an abscess of the broad ligament pointing towards the groin, and which, I think, will discharge itself externally in a short time. Some years ago I had a case of a woman who had abscess pointing and discharging in the groin on two occasions, and after two consecutive confinements.

DR. LAWLOR.—In the institution over which I preside there is a tendency to low inflammation, and there is one very severe case of gangrenous erysipelas.

Syphilitic and other Tumours of the Labia and Clitoris. By M. A. BOYD. F.R.C.S.I., M.R.I.A.; Surgeon to St. Michael's Hospital, Kingstown.

MR. PRESIDENT AND GENTLEMEN,—The cases which I beg to bring under your notice this evening are records of some that have come under my own observation from time to time, and I deem them of sufficient importance to lay before the Society. In nearly all the handbooks on diseases of women the diseases of the external organs of generation are only noticed in a cursory manner, a mere glance being given at a great many that are by no means rare, and of some there is no mention made at all. This may be in a great measure owing to many of these diseases in themselves causing so little inconvenience to those who suffer from them, and in general interfering so little with their general health that delicacy alone may prevent their subjects making more frequent mention of them.

I think the diseases involving these parts of exceeding great interest to the obstetric surgeon, and by no means so rare as most authorities would lead us to suppose from this desultory mention of them. They are certainly not of so much importance as diseases of the cervix and uterus as to merit a primary place in our consideration; yet some of them, by their long continuance, exercise such a prejudicial effect on the mind and spirits of the patient as in the end to affect the general health, and call for operative or other interference on the part of the surgeon.

The first case I will call your attention to is that of tertiary syphilitic enlargement of the clitoris, and though a good deal of doubt surrounded the diagnosis of the case in the beginning, the evidence of syphilitic contamination of the system was found to be so manifest that none could be entertained as to the nature of it ultimately.

Eliza F., aged thirty-eight years, a widow without children, and menstruating regularly, was admitted to St. Michael's Hospital, Kingstown, in September last, with an ulceration of the leg, of long standing; and from the shape of the ulcer and character of the discharge, I concluded the case was one of tertiary syphilitic ulceration. As there was no evidence to support this in the history she first gave of her case, I was in a good deal of doubt, but while under treatment she called my attention to a tumour on the external genital organs, from which she suffered for some years. On making an examination, I found an enormous tumour springing from, and continuous with, the clitoris, which organ was thickened to the size of the index finger, and formed its pedicle, and from this it branched out into lobes and tuberculated masses with deep fissures between, exactly like a cauliflower in shape. It was of a pale pink colour, and when the pedicle was compressed became of a livid tint; round the labia majora and perinæum were several smaller tuberculated growths, and the integument between them had a brawny, greasy appearance. There was continual incontinence of urine, which was a source of great annoyance to the patient, and resulted from displacement and elongation of the meatus, due to the great weight and dragging of the tumour; it was dilated to three times its natural size, and the finger could easily pass through it into the bladder. On closer examination there was detected on the legs and body of the patient the remains of a tertiary syphilitic rash. She first noticed the enlargement of the clitoris about five years ago, previous to which she contracted gonorrhœa from her husband, but did not remember having had sore throat or any rash subsequent to or prior to that period. There was a continual thin watery discharge from the fissures between the lobules of the tumour, and occasionally pruritus of the vulvæ.

As there could be but one opinion regarding the treatment of the case—namely, extirpation of the entire mass, I placed the patient under chloroform, and with the knees well drawn up and everted I transfixed the pedicle of the tumour with a hair-lip pin, and, carrying a needle armed with a stout silk ligature between it and the base of the pedicle, strangulated it in two segments. I then, with one stroke of a scalpel, cut away the tumour in front of the needle, which I allowed to remain on the stump. Not a drop of hæmorrhage followed. The patient complained of a good deal of pain after the operation, which was relieved by a morphia suppository and some carbolic oil, and lint was applied to the cut surface. On the fourth day both needle and ligature came away, and at the end of ten days the raw surface was entirely healed. The patient was put on a course of bichloride of mercury and bark, and in the course of a week or fortnight was able to leave the hospital, and resume her usual occupation. The incontinence of urine had also disappeared.

Had I a sufficiently strong battery near me to heat the wire, I would most certainly have removed the tumour by the galvanic ecraseur, as the case was a most favourable one for operation by this method.

By the introduction of the hair-lip pin through the pedicle, I insured the ligature against the danger of slipping off, and was enabled to remove the tumour at one operation. The usual rule in tumours of the kind is, I believe, to strangulate their base, and leave them to slough off; but I need hardly say how sickening to a patient and disgusting to the attendants the decomposition of a tumour of this kind would be removed by such a method. The tumour was elastic to the feel, and quite pain-less in its growth from the beginning; it weighed over six ounces.

A case somewhat similar to the one just related came under my notice some years since in the Rotunda Hospital, the notes of which I have preserved.

Mary H., aged thirty-five years, two years married, was admitted to the Rotunda Hospital suffering from pains which she considered were due to premature labour. She was about six or seven months pregnant, and contracted syphilis from her husband two years previously. On attempting to make an examination it was found impossible to introduce the finger into the vagina, the orifice of which was almost entirely occluded by a hard tubercular mass as large as an egg, which was found to spring from the margin of the perinæum and left labia; it had been growing for six months previously, and was rather painful to the touch; it differed from the case already described in being sessile in its attach-ment, quicker in its growth, harder to the touch, was of a purple appearance, and deeply cracked and fissured, and considerably œdematous; the cracks also gave exit to a puriform, very fœtid discharge. There was an extensive copper-coloured rash all over the body, and the patient had all the glandular engagements of secondary syphilis. As there were no immediate symptoms to indicate the approach of labour, an operation was spoken of for the removal of the growth, as it would offer a most serious impediment to the descent of the head, but the patient would not consent to it, left the hospital, and I afterwards lost sight of her. That this growth was the product of secondary syphilis there could be no doubt, and whereas the former tumour approached more nearly to the hypertrophic character in the painlessness of its growth and soft integu-mental feel, and in its colour, which was little different from that of the clitoris in its normal state, the latter, from its angry inflamed appearance and puriform discharge, showed more of the indurated character of secondary syphilitic growths.

Warty growths springing from the labia, clitoris, or margin of the perinæum I always look upon as of syphilitic origin, though this is doubted by some authorities. I have never seen warts springing from these situations in any case that there was not the most conclus----

evidence of their being syphilitic. I will certainly allow that warts may spring from any portion of the cutaneous surface, but when once they invade the mucous membrane of this situation, or spring from its margin, we may at once stamp them as being syphilitic. They may sometimes spring from the integument here when they are general on other parts of the body, and non-syphilitic; but when we find them here and absent elsewhere, we may conclude they are syphilitic, and may make this fact the grounds of our diagnosis. They generally grow in clusters or bunches, are very vascular and elongated, and cause a very considerable muco-purulent discharge, and occasionally pruritus of the vulva. With regard to their treatment, I have found simply powdering them over with calomel once or twice a day sufficient for their removal when not very large or numerous. If this is not successful, the dried sulphate of zinc used in the same manner will be found to succeed, snipping off with a scissors those that are very long or hard.

Primary syphilis, or hard chancre, which almost always occurs on one or other of the greater labia, is so characteristic in its appearance, and accompanied by so much induration of its base, that I need not here describe it in detail. Every practitioner is so familiar with it that it would be impossible to confound it with anything else. The induration is oftentimes the only symptom we find present—the ulceration that accompanied it in the beginning having healed previous to contamination of the system.

Simple hypertrophy of the clitoris I have often seen without there being the least grounds for believing that it was in any way connected with syphilis, but to what cause it may be due I am at a loss to explain. It sometimes causes very troublesome pain and soreness, especially after much exercise. Œdema of the clitoris and prepuce I have frequently seen associated with an acute attack of urticaria or eczema of the body. Cooling and astringent lotions, such as the lead lotion, or sulphate of zinc with watery extract of opium, seldom fail to give relief. Simple hypertrophy of one or both labia minora occurring in this country without any syphilitic taint of the system has been noticed by different writers, and a case of it came under my observation some time since.

Mary D., an unmarried woman, aged thirty, consulted me two years ago for a swelling of the right labia, from which she suffered for three years previously. It did not cause her much uneasiness, unless after much exercise, when it became acutely painful and swollen. It was gradually enlarging from the time she first noticed it till she consulted me. On examining her I found the labium minus extended an inch or an inch and a half beyond the external labium; it was slightly œdematous, but did not differ in consistence from the labium of the opposite side, which was normal in size; the mucous membrane covering its inner aspect was rough and granular to the feel, and there was considerable

leucorrhœal discharge, but there were not the slightest symptoms of syphilis in any form. After using various astringent applications for some time without any effect, I put the patient under chloroform, and by two longitudinal incisions through its base removed the entire labium. A small artery supplying the base of the growth spouted after its removal, the hæmorrhage from which was easily arrested by torsion. Having brought the lips of the wound together by a few points of silk suture, the wound healed in a few days. I saw the patient some months afterwards, and she said she felt quite cured, the soreness had altogether disappeared, and she was able to take any amount of exercise without inconvenience. Cases of this kind are rare in this country, but among the negro women the labia minora sometimes attain an enormous size by simple hypertrophy, cases being recorded by trustworthy writers where they were so elongated as to reach the knee.

The next class of tumours I beg to call your attention to are vascular tumours of the meatus, and of all the morbid growths that attack the external genital organs of women, these are probably about the most numerous, the most troublesome to the patient, and the most creditable or otherwise to the surgeon in the relief afforded by their removal. I know of no morbid growth (and in this I think I will be borne out by the majority of the members of this Society) which for its size causes so much pain, which generally occurs in an inverse ratio to its size, for the smaller the tumour the greater the pain, or one that can be more easily overlooked by a careless or insufficient examination, for the tumour when small is often entirely hidden from view, and it is only by paying attention to the uneasiness a patient exhibits in making an examination that we will be able to light on the exact situation of the cause of the suffering. These tumours I have seen occurring in women of all ages, but most generally in women who had passed the climacteric period. The first symptom the patient generally complains of is a desire of frequent micturition, with a sense of burning or scalding following it, and suprapubic and lumbar pains. I have seen in a great many instances the true cause of these symptoms overlooked, and the patient treated for vesical irritation, or urinary concretions in the bladder, when a careful examination would at once indicate the nature of the disease. On attempting to make a vaginal examination in a case of this disease, you will find the moment you begin to introduce your finger the patient will generally start and cry out with pain, and the moment you come in contact with the orifice of the meatus or the tumour contained in it the suffering becomes sometimes intolerable. On everting the labia and examining the meatus, you will find a small red granulation, which the slightest touch will bleed, filling up its orifice, sometimes exceedingly small, sometimes as large as a strawberry, which it resembles very much in appearance.

With regard to the pathology of these growths very little has been added to our knowledge since Clarke so accurately described them; they seem to be true vascular tumours or arterial nævi, highly endowed with nerve filaments and bear some, resemblance to the vascular hæmorrhoid of the rectum described by Houston. The plans of treatment for the extirpation of these vascular tumours are various—drawing them out with a forceps, and snipping them off with a scissors, seems to be the one most generally approved of, but the hæmorrhage after this method is sometimes very considerable, unless some caustic or the actual cautery be applied afterwards, both of which are exceedingly painful. Nitric acid has also been recommended, but its application is most painful, and it is difficult to protect the surrounding parts from injury. Of all the methods of treatment I am acquainted with, I know of none more effectual or less painful than the galvanic wire or cautery. By drawing them forward out of the meatus with a forceps, and encircling them with the wire, their removal is only a matter of seconds, and neither pain nor hæmorrhage follows the operation.

Fatty, fibrous, or cystic tumours of the external labia, though generally very rare, are sometimes met with. I saw not very long since a case of fatty tumour of the right labia. I was called to see the patient for retention of urine, and when about to pass the catheter discovered a tumour about three inches long springing from the right labia. The subject of it was an unmarried woman about forty years of age, and the tumour had been growing for over six years. When she first noticed it it was about the size of a small marble, and it gradually and painlessly continued to increase ever since; it was pedunculated, and had the usual feel of a fatty tumour, and puncturing with an exploring needle confirmed its nature. I advised the patient to submit to its removal, but she declined, as it caused her so little inconvenience. Fibrous tumours of the labia I have never seen, but several such tumours have been described and removed by various authors. They sometimes attain a very large size, and their diagnosis is in general not difficult. When they are allowed to grow for some time without interference, their tendency is to become pedunculated, when their removal is rendered easy by either knife or ecraseur.

At the last meeting of this Society a very interesting tumour of the labia was exhibited by Dr. Atthill; it was of enormous size; began as a soft elastic swelling in the left labia, about the size of a walnut, when first noticed about a year previously. It was an encysted tumour, and belonged to the class known as myxomatous. It is a very unusual form of tumour.

Abscess of the labia I have frequently seen; they generally occur in the sulcus between the large and smaller labia, and result from blocking up of some of the sebaceous glands in this neighbourhood, followed by

inflammation and suppuration. The patient first generally complains of pain after exercise, with a sense of burning or throbbing, followed by swelling of the external labia, and pain in the groin of the affected side, the glands of which may be enlarged. I have seen it as often in un-married as married women, and it invariably results from want of cleanliness. I have also seen it result from scabies or eczema of the pudendal region. When the abscess forms, puncturing it with a lancet, followed by warm fomentations and poultices, with rest in bed, will effect a cure in a few days.

Cystic tumours of the labia are not at all unusual, but in their growth are so painless and so little inconvenient that the surgeon is seldom con-sulted regarding them. I have met them in four cases, in all of which they sprang from the margin of the greater labia close to the perinæum; none of them attained a larger size than that of a good-sized marble, and the smallest was not larger than a pea. In one case, that of a married woman, aged forty, the tumour was the size of a large marble, sprang from the inner side of the greater labium on the right side, and was of four or five years' growth. On puncturing it with a grooved needle, which I did after a careful examination, it gave exit to a thick glairy mucus. I then opened it up by a long incision, and cauterised its cavity with the solid nitrate of silver. Some suppuration took place, which continued for a considerable time, but ultimately ceased, and the tumour disappeared, leaving only a small puckered cicatrix to indicate where it had been. These cysts sometimes attain a very considerable size—cases being on record where they have attained the size of an orange; they are always painless in growth, are very superficial, and become troublesome only by their size. After tapping they generally fill again, like all cystic tumours, and are best dealt with by being entirely removed when not very large. Cutting a slice out of the cyst wall is another method that has been recommended for their removal. This is certainly a very good method when the tumour is large, and when some irritant is applied to their cavity in addition, such as nitric acid or nitrate of silver. They may be sometimes confounded with a hernia, and the practitioner should by a careful examination ascertain the true nature of the swelling before he has recourse to any operative interference. With regard to their pathology, they are due to obstruction to some of the mucous follicles which exist in this situation.

Malignant disease attacking the external organs of generation would seem to be rather rare in comparison to the number of cases of this disease that are met with affecting the cervix and vagina. Though I have met with several cases of the latter among the extern patients of the hospital, I cannot say that I ever met with a case of scirrhus beginning in the labia or clitoris. Cases of this form of cancer are, however, recorded by different authors. It generally begins in either nympha as

a painful tumour, of stony hardness, preceded for some time by trouble-
some pruritus of the vulva, which would seem to be its most characteristic
symptom, and it involves the clitoris secondarily. All the authorities
who have described it have met with it principally in one or other labia,
and the subjects of it were all women who had passed the middle period
of life, and showed in conjunction the cancerous cachexia. When seen
early the only chance of prolonging the patient's life is extirpation by
the knife, as caustics in any form of cancer are both painful and unsuc-
cessful in removing the entire disease.

Epithelial cancer rarely, if ever, attacks the nymphæ or clitoris, and
seems to confine itself entirely to the mons veneris or external labia. I
saw a case of it some time since, the notes of which I preserved. Alice
T., aged sixty, the mother of four children, all living, consulted me
for a swelling and enlarged suppurating glands in the groin, the dis-
charge from which was very fœtid. On examining her I found the
right labia almost covered by a round epithelial cancer, with indurated
edges and rough, warty-looking granulations, from which there was very
little discharge. It began as a small wart about five years previously,
which gradually enlarged—the glands in the groin becoming enlarged,
and ultimately suppurating, five or six months previous to my seeing her;
a large hernia could also be felt in the groin above the suppurating
gland, which escaped from the abdomen under Poupart's ligament,
through the softened and ulcerated parietes. The patient shortly after-
wards died, worn out from exhaustion and suppuration—any operative.
interference being, of course, out of the question. Had I seen the
patient early before the glands of the groin became affected, I have no
doubt excision of the epithelial growth would have been successful in
saving her life.   .

The President.—This is a very comprehensive paper, and embraces
a great number of diseases affecting the genital organs of the female.
One important point involved is as to the diagnosis of diseases of a
syphilitic nature as distinguished from those of a more malignant character.

Dr. Atthill.—Dr. Boyd's interesting communication embraces nearly
every variety of labial tumour that we meet with. As to the syphilitic
forms, I am not very familiar with them, and I never had a case of this
kind calling for operative interference. I have seen a good many cases
in which the labia minora were hypertrophied and enlarged, apparently
the result of syphilis, but in none of them did I find surgical interference
necessary. Dr. Boyd is to be congratulated on the success which has
followed his operations. Ten or twelve years ago I laid before the
Society the particulars of a case in which I removed the entire of the
left labium and the greater part of the mons veneris for what I deemed

to be elephantiasis of that organ. This occurred in an unmarried woman. The tumour thus formed overlapped the orifice of the vagina, giving rise to pruritus of so distressing a nature as to be unbearable, and the woman willingly submitted to the operation in order to obtain relief. She subsequently married, and became the mother of several children. I have never since seen a case of elephantiasis of the labia majora. Enlargement of the labia minora is not so uncommon, and several cases have come under my observation. One was that of a young lady who has since gone out to India. At every menstrual period the labia minora became very pendulous, and were congested and enlarged to such a degree that she was willing to submit to the operation of the removal of the parts. However, she got a great deal better from rest and the use of astringents and lotions, and operative interference became unnecessary. What results have since followed I do not know, but I rather fear the heat of the climate of India will aggravate the symptoms. Not long since I was consulted by a young lady who was greatly distressed because she supposed herself to be deformed. On examination I found an enlargement of the labia minora, not to any great extent, but sufficient to give rise to the idea that she was not rightly made. I had also under my care recently a widow in whom one of the labia was of immense size, soft and pendulous. She did not complain of this, and therefore I took no notice of it. She consulted me for uterine disease. Had there been any annoyance I might have thought of amputation, although that operation is one about which I should hesitate, as the organs are very vascular. The little vascular growths springing from the orifice of the urethra sometimes are very troublesome. Sometimes the patient does not complain at all, while in others—such as that described by Dr. Boyd—a very small growth gives rise to most distressing symptoms. I believe the galvanic cautery to be one of the best possible means of extirpating these, but it is a little troublesome of application, and is not always at hand. In one case I removed one satisfactorily by means of Sir William Wilde's aural polypus snare. Lately a little speculum for the urethra, made of boxwood or ivory, has been introduced into practice. It is like an anal speculum in miniature, and is very useful. By means of it you can isolate the growth, and can then apply either a strong caustic, such as nitric acid, without touching the walls of the urethra; but, as I have already said, in a considerable number of these cases there is no irritation complained of, and then I let the patients alone. I do not consider nitric acid to be efficient, and in these cases I have seen it totally fail.

Dr. Kidd.—Very frequently the tumour is not confined to the meatus, but extends the whole length of the urethra, for which reason the simple removal of the tumour will fail. You remove what projects out of the

meatus, but you do not get at the seat of the disease. I have used the boxwood specula, which, I think, were invented by Professor Simont, of Heidelberg, who died lately. I do not think the nitric acid fails so frequently as Dr. Atthill seems to think. If you scrape the surface and excise the tumour with a fine scissors, and then apply the nitric acid, it succeeds according to my experience. The specula are very convenient for that purpose.

DR. ATTHILL.—Did Dr. Boyd ever inject any cystic tumours of the labia with iodine?

DR. KIDD.—I injected a cystic tumour of the vagina. The case is recorded in Dr. M'Clintock's book. The tumour was the size of a Tangier orange. It did not succeed, but I cured the tumour afterwards by laying it open and dressing it from the bottom.

DR. CRANNY.—I saw a case of cystic tumour cured in the Rotunda Hospital by an injection of iodine, and I have since cured a case of the sort myself in a similar manner. The case cured in the hospital was one of a syphilitic growth, similar in character to the tumour shown this evening by Dr. Boyd, but in it both sides of the labia were involved. I think the case was brought before this Society by Dr. Johnston. He effected the removal with an ecraseur. I used nitrate of mercury, which, when applied to a few small points, seemed to have a good effect in obliterating the tumour. There was such a large growth, however, that there was nothing for it but removal, and the case ended very well. One very remarkable circumstance about the case was that the woman had been inoculated with syphilis twenty years before the operation.

DR. M'CLINTOCK.—In Dr. Atthill's museum in the Lying-in Hospital there is a preparation of enormously enlarged nymphæ and clitoris. The organs had been removed from a woman in the chronic ward of the hospital, and in each labium is a small tumour which bears the very closest resemblance, both in size and appearance, to that exhibited by Dr. Boyd to-night. The clitoris was enlarged to the size of an enormous pear. The disease was distinctly traceable to syphilis, which the woman got some years after her marriage. In the clinical memoirs to which Dr. Kidd has alluded there is a full history and illustrations of this particular case, as well as of some others on which I operated for syphilitic enlargement of the nymphæ and the clitoris. Cases, however, sometimes occur in which diagnosis is very difficult. I have recorded in the same chapter a case of a young lady who was at the time under the care of Dr. Stewart, of Lucan, and who had an immense hypertrophic enlargement of one nympha, and if I had seen it without taking into account

the concomitant circumstances, I should certainly have pronounced it to be a syphilitic enlargement. In this case, from the social position of the patient, the history of the case, and all the surrounding circumstances, it was to the last degree improbable that there could have been any syphilitic contagion whatever. That enlargement was removed by an operation. I mention this case to show that we ought not too hastily to attribute a syphilitic nature to those tumours merely from their appearance. I do not insinuate that Dr. Boyd has laid down such a principle of diagnosis at all, for he has frankly stated in his paper that in some of these cases the circumstances indicate a hypertrophic enlargement and not any syphilitic origin. As to the vascular tumours of the meatus, I have generally found the treatment by nitric acid to be most successful and less painful than that by nitrate of silver. The pain caused by the former, though very acute, is transient, which is not the case with the pain caused by nitrate of silver. Dr. Boyd has certainly given us to-night a very interesting and complete *résumé* of the various forms of tumour, enlargements, and growths that are to be met with in the external pudendum.

DR. FITZPATRICK.—I fully agree with Dr. Boyd that, as a general rule, warts of the nymphæ are syphilitic. I once attended a young woman, only six months married and pregnant, who complained of a soreness of the vagina, and on examination I found the whole nymphæ to be covered with a crop of warts. Suspecting syphilis, I spoke to her husband, who emphatically denied that he had ever had either syphilis or gonorrhœa in his life. Dr. Beatty saw the case with me, and under his guidance we tried nitric acid, sulphate of zinc, and in fact every other possible mode of treatment, in order to remove the warts, but in vain. At last Dr. Beatty said he feared it was a case of syphilis, and recommended me to examine her husband. I did so, and found no traces of those diseases whatever in him. The man was apparently honest and truthful, and told me that if he had those diseases he would not hesitate a moment to tell me so; but on drawing back the prepuce I saw one little wart that had very much the appearance of a louse, it was so small. I then arrived at the conclusion that the warts were of syphilitic origin. The lady never had anything in the way of a sore or any discharge, but Dr. Beatty and I came to the conclusion that the only mode of cure was to put her under a course of mercury, and as soon as she had been subjected to that treatment the warts disappeared. She was confined, and had a healthy child; she never showed any secondary or tertiary syphilitic symptom; and the case is one of those puzzling ones that I cannot to this day satisfy my mind about. But that her husband was perfectly honest in his statement, that he had never had either syphilis or gonorrhœa in his life, I quite believe.

Dr. Boyd, in reply, said—I do not think I have anything to add. I simply recorded the cases as I met them, and I was very glad to hear the remarks made upon them. Dr. Atthill wishes to know if I treated any of the cysts with injection of iodine. In the case recorded in my paper, after having laid open the cyst and turned out its contents, I rubbed it with nitrate of silver. This caused suppuration, which resolved the cyst into an abscess, which closed up after running for about three weeks. I have injected chronic abscesses with tincture of iodine, and found good results to follow, but I never tried it in a case of cystic disease of the labia.

The following communication was read by Dr. Kirkpatrick:—

On Strapping the Breast as the best means of Arresting the Secretion of Milk after Stillbirth. By W. J. Wilson, L.R.C.S., Edin.; Assistant-Surgeon, U.S. Army.

<div align="right">Bagnalstown, Co. Carlow,
21st November, 1876.</div>

To the Hon. Secretary, Dublin Obstetrical Society.

Sir,—I take the liberty of sending you this communication, and asking you to be kind enough to lay it before the Obstetrical Society, for the comments and criticisms of the members on the mode of treatment described therein.

About February, 1875, in *The New York Medical Record*, I saw the account of a discussion in the Obstetrical Section of the New York Academy of Medicine on the best method of preventing the secretion of milk after stillbirth, or when from any cause it becomes necessary to arrest that secretion. Having had a few cases in which I adopted a different mode of treatment to any of those mentioned in the remarks of the members, I wrote to the presiding officer on that occasion, Dr. E. R. Peaslee, Professor of Obstetrics in the Bellevue Hospital Medical College, New York, giving him an account of the following cases:—

In March, 1867, while in private practice in the State of Missouri, I attended a lady who had a miscarriage about the sixth month of pregnancy. In the course of a few days the breasts began to fill, and though I tried all the remedies I then knew of to "put back the milk," such as ext. belladonna, iod. potass., &c., &c., and consulted with the other practitioners in the town, and tried their modes of treatment, all were without avail. The breasts continued to increase in size, and the outer and lower portion of the right breast began to assume a livid colour, and to "pit" on pressure. While they were in this condition—and I dreaded that an abscess would surely form—the thought struck me that strapping might be useful. I accordingly procured several straps of the common adhesive plaster, about five-eighths of an inch in width and eighteen or

twenty inches in length, and, passing the first round the base of the breast, brought one end across the top of the sternum and the other across the outer end of the clavicle. Other straps I passed in a similar manner, each slightly overlapping the other, until the breast was nearly covered with the plaster. I then strapped the other breast in like manner. The relief to my patient was instantaneous; the dragging feel was at once relieved, and she felt very comfortable. Next day I removed these straps, as they had become slightly loose, replaced them with others, and continued this mode of treatment for about four days, when the enlargement of the breasts, the tendency to abscess, had entirely disappeared, and they had resumed their normal size, appearance, and condition.

In September, 1867, I attended a lady in her confinement at the natural term, and about a week afterwards she spoke to me about her right breast, saying she was afraid she was going to have some trouble with it, as after a previous confinement the nipple of that breast had become closed. I applied the adhesive plaster in the manner described above, and in a few days the milk had entirely disappeared from it, and it had resumed its normal condition.

In October, 1868, I attended the wife of an army officer who miscarried about the fifth month. When the breasts began to fill I applied the plaster, and in a few days all trouble was over.

These cases I published about July, 1870, in *The Philadelphia Medical and Surgical Reporter*, as a method to prevent the formation of mammary abscess.

Just about the time I saw the report of the discussion in the New York Academy of Medicine, I attended a soldier's wife at the regular time, but whose child was born dead, as it was a breech presentation. "Here," said I to myself, "is a case in point." So when the breasts filled I strapped them carefully each morning for four days (not having given her even a dose of castor-oil or any other medicine whatever), when the milk disappeared, and the breasts had assumed their natural size and condition.

After this case I wrote to Dr. Peaslee, drawing his attention to the discussion, and giving him, as I have you, the history of the above cases. He wrote me a very kind letter in reply, thanking me for my communication, and saying that he would bring it before the Academy of Medicine and his own class of over 500 pupils at the Bellevue Hospital Medical College.

After waiting for a couple of months, and not seeing any account of it in the reports of the meetings of that body, I again wrote to Dr. Peaslee, and below I give you a copy of his reply :—

"29, MADISON-AVENUE, NEW YORK,
"*June 26th, 1875.*

"DEAR DR. WILSON,—I intended long ere this to have replied to your letter of the 11th ult. My absence on a journey to the South prevented me from bringing the subject of your first letter before the Societies, as I had intended, though I spoke of it to several obstetricians here just as I was about leaving. Since my return I have been so pressed as not to be able to be present at the meetings, as I had expected. But at the meeting for this month of the Obstetrical Section of the Academy of Medicine, your letter was formally presented, and the subject discussed. No one present but myself had ever used plaster as ordered by yourself. I have done so in five cases since I received your first letter, and in every instance with perfect success. I had two cases requiring some method to prevent the secretion of milk in a single day the 4th of this month.

"All were pleased with the idea of strapping as you propose, and voted unanimously to thank you for your interesting letter, and to insert it in the records of the Society. I shall bring up the subject again next fall in other Societies, as they have all now adjourned through the summer.

"With many thanks for your valuable communication,

"I am, yours very truly,

"E. R. PEASLEE.

"DR. W. J. WILSON, Assistant-Surgeon, U.S.A."

The formal vote of thanks was conveyed to me in a letter from the Secretary of the Society, Dr. Sylvester Card.

I do not claim to be the originator of this mode of treatment, but have been unable to find any mention of it in the various works on midwifery that I have had access to. I send it to you for what it is worth, and will be glad if you bring it before the Obstetrical Society.

Respectfully yours,

WILLIAM J. WILSON, L.R.C.S., Edin.;
Assistant-Surgeon, United States Army.

I should have brought this subject under the notice of the Society before this time, but having got leave of absence from the United States, I entered the Egyptian army, and accompanied the late Egyptian Expedition to Abyssinia as Surgeon-in-Chief of that Expedition. At the battle of Gura, on the 7th March last, I was severely wounded, having been shot through the left leg about three inches above the ankle, fracturing both bones. I am now slowly recovering from the effects of that wound, and expect to be able to return to the United States in January next.—W. J. W.

The PRESIDENT.—The author of this communication is not present, but I shall be very happy to hear any remarks on it. I think you will all admit that the suggestion is not anything very new. I think I remember, when I was rather a younger man than I am now, seeing straps applied sometimes with great benefit for preventing the formation of milk.

DR. KIDD.—It seems to me that the cases detailed in the communication divide themselves into two classes. In the first the breast had become prominent, pitted on pressure, and was livid on the surface, and I think we can scarcely doubt that matter had formed. In the other cases the strapping was used for the purpose of preventing the secretion of milk. I think we have all known for a long time the use of strapping in cases of inflammation of the breast. The practice is one older in this country than my recollection; but the employment of strapping for preventing the secretion of milk is, I confess, to me something new. Whether it is a good practice or not is very doubtful. In this country I think the usual course is to use what are called cere-cloths, made by the nurses, and applied from time to time. Most patients have found them very comfortable, and if the breast is not rubbed and handled too much, I think under the use of the cere-cloth the milk disappears without much inconvenience. In London the usual practice seems to be to apply to the breasts layers of lint wet with some spirit lotion, such as eau-de-Cologne, and over these to place oiled silk or some india-rubber cloth. I have never used that application, but I believe the patients find it comfortable enough. A few weeks ago I attended a lady who had been confined in London, and had had her breasts treated in that way. After she came under my care I used the old-fashioned cere-cloths, and asked her which was the most comfortable. She could hardly tell, but on the whole preferred the cere-cloths. We sometimes read of evaporating lotions being applied to the breasts. That is a mode of treatment which, I think, is a rather dangerous one. Strapping for chronic or sub-acute inflammation of the breasts, as we all know, has been common enough, but the point of the present paper is strapping for the prevention of the secretion of milk. The suggestion is new to me, and is worthy of being taken into consideration, although I am not quite sure that I should be inclined to adopt it.

DR. DENHAM.—In my opinion the best cure for all these conditions of the breast is "tincture of time"—to let them alone. Great evil arises from nurses using breast-pumps, and attempting to draw milk out when there is not the slightest necessity for it. My opinion is that those cases should be left to nature without any treatment whatever, and, if so, they will get well in a day or two without any bad result. Unfortunately,

however, the public are so impressed by the idea of the importance of treatment in every case that we must do something, and then we put on cere-cloths and do other things, whereas my decided conviction is that in most cases of the sort nature and a little cooling medicine would work the cure. Gentle rubbing with a little almond oil I believe to be useful. The idea of strapping was borrowed from the practice in cases of swelled testicle in cases of gonorrhœa.

The Society then adjourned.

THE INFLUENCE OF FARADISATION ON AN ENLARGED SPLEEN, AND ON THE PAROXYSMS OF INTERMITTENT FEVER.

B. SKORRZEWSKY (*Wiener Med. Wochensch.*, 1876, No. 21), believing the enlargement of the spleen in ague to be due in chief to a depressed action of the vaso-motor nerves, employed Faradisation to diminish the size of the gland. He placed the electrodes, one over the anterior, the other over the posterior border of the spleen, and gradually increased the intensity of the current. Each sitting lasted 15 to 20 minutes. The results he arrived at are as follows :—The induction current produced a decided effect in reducing the size of the tumour; the first application was generally followed by the most marked alteration ; the softer the spleen the more striking was the effect of the electricity. The spleen became hard on palpation, and the pain and uneasiness in the side were alleviated. The reduction of the gland, which was always best marked in the first few days of this treatment, was in rare cases noticed as early as 24 hours after its application. No relapses occurred in any recent case of ague with a swollen spleen in which Faradisation had been carried out. The benefits of the electrical treatment were essentially aided by the administration of quinine. As to the influence of electricity upon the attacks of ague there was no constancy in the results; for, while in six out of ten cases they seemed to be ameliorated, in four there was not the least effect noticed. It was further observed that, while under treatment, the type of the intermittent changed, and the paroxysms were either anticipated or postponed. The general health of patients seemed to be improved while subject to this treatment ; and in one case albuminuria and dropsy were removed by it. Dr. Litten, who has reviewed the foregoing paper in the *Centralblatt*, No. 42, 1876, states that he had employed similar treatment in a considerable number of cases since 1874, but that in his hands it was not followed by any constant diminution in the size of the spleen.

J. M. F.

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

President—RICHARD ROSS, M.D.

Honorary Secretary—WILLIAM WHITLA.

Tuesday, December 26, 1876.

DR. ROSS, President, in the Chair.

A Successful Case of Ovariotomy. By DR. MURNEY.

DR. MURNEY described a case of ovariotomy, performed by him on the 23rd November, resulting in recovery, the wound being almost entirely cicatrised, and the patient out of bed on the 24th day from operation.

The leading facts of the case were :—A widow, aged thirty, otherwise healthy looking, who had never been pregnant, seven years since noticed a small movable tumour in the left side of the abdomen. From May, 1876, this rapidly enlarged. Early in October she had acute pain in the abdomen, which continued for several days, and which compelled her to seek admission into hospital. For some time prior to operation the pain had subsided, but the abdomen was so large she could not approach the recumbent posture.

23rd. Nov.—The patient having been chloroformed, an incision was made from a short distance below the umbilicus to about one-half inch above the pubis. On exposing the tumour its anterior surface was found to be adherent to the parietal peritoneum. These adhesions were broken down by the finger and the sac punctured by the canula. Twenty pints of the usual ovarian fluid were drawn off. The diminished mass was now readily drawn through the wound, as there were no adhesions binding it posteriorly. A Liston's needle, armed with dentist's silk, was passed through the pedicle (which was short) and the two ligatures tied, then another ligature was carried round the two, the pedicle cut, and the uterus allowed to fall back into the pelvis. The ends of the ligatures were carried out through the lower part of the wound. The right ovary was healthy. Six sutures brought the incision together. A pad of lint, saturated in carbolic oil, laid on, then a dry pad and a flannel roller. A suppository of half a grain of hydrochlorate of morphia administered, and repeated at bedtime for eight days, when one quarter grain was used for four nights. After this no further anodynes were required.

On the fourth day there was slight action of the bowels without medicine.

On the fifth two sutures were removed, and menstruation commenced and continued for three days.

On the sixth the remaining sutures were taken out.

On the twentieth the ligatures came away.

On the twenty-fourth she was out of bed for the first time.

	Temperature		Pulse	
	9 a.m.	9 p.m.	9 a.m.	9 p.m.
23rd Nov., ...	98·4°	100·8°	86	92
1st day, ...	100°	100·8°	104	120
2nd day, ...	99·2°	98·4°	110	115
3rd day, ...	99·2°	99·2°	108	100
4th day, ...	98·2°	98·4°	100	100
Normal afterwards.			Steadily came down.	

Measurements of Abdomen.

	Before Operation.		After.
	30 days	5 days	26 days
Circumference at umbilicus, ...	39½	41	28
Pubis to umbilicus, ...	7½	9	4½
Pubis to ensiform cartilage, ...	17	19½	10
Umbilicus to ensiform cartilage, ...	9½	10½	6
Umbilicus to anterior superior spine, on each side, ...	9¾	10½	5

In addition to the large sac containing the fluid which was drawn off, the tumour was made up of four other sacs, small, and the contents were much more viscid and much more opaque than the first mentioned.

DR. J. W. BROWNE congratulated Dr. Murney upon what he said was the first known case of ovariotomy in the North of Ireland. Other cases, he believed, had been successful; but this was the first published one. He detailed two fatal cases in the Belfast Royal Hospital. He approved highly of the details of the operation, which he would exactly follow, were he called on to operate. He censured Dr. Wells' trocar and canula as a delusion. He saw it used in three cases, and in all it failed in its object. He did not think that tapping a few days before the operation would lessen the shock to the patient's system, nor did he think it would facilitate, in any way, any step of the operation.

DR. JOHN MOORE, in congratulating Dr. Murney upon the success of his operation, referred to the successful case of Dr. Thompson of Lisburn— the first, he stated, in the province of Ulster. He had thought this a most suitable case for operation, and so it had turned out. As regarded

tapping, he thought that tapping before operation would prevent the surgeon distinguishing between the cyst and the peritoneum—a most important point. He was present at the operation, and would say there was not a detail upon which he thought he could improve.

Drs. Spedding and Wheeler saw no advantage in tapping.

Dr. M'Keown reviewed the history of the operation, and referred to the great success of Spencer Wells and others. As regarded the remarks of Dr. Browne, he thought that when a man of such vast experience in ovariotomy as Wells—experience which could not be hoped to be within the reach of any present—approved and used constantly in his practice any instrument, he thought they were called upon to follow him.

Dr. Fagan congratulated Dr. Murney, not so much on the success of his operation, as for his superior diagnostic powers, which were well shown by his selection of so suitable a case for operation. The mere mechanical quality of doing an operation well, was, he held, no test of true ability in a surgeon, while acute diagnostic powers entitled him to the highest rank. Why it was that many operations of great merit had fallen into disrepute was, he believed, due to the bad selection of cases for operation—not to the operation itself. He was present at an operation for ovariotomy a few years ago at a London hospital. The patient was under the operation for two hours or more. The cyst was adherent throughout. The surgeon, in the most dexterous and fearless manner removed it, and the patient died two hours afterwards. It then occurred to him had the surgeon been as acute in diagnostic powers as he was dexterous and fearless in the use of the knife, he would have rejected it as unsuitable for operation. The opening of the abdominal cavity should not be considered, as it usually was, so formidable an operation. He saw every week in the journals reports of successful cases of abdominal section for cases of intussusception, of ovariotomy, and, occasionally, of Cæsarean section. When suitable cases were selected, and ordinary care taken in its performance, he believed ovariotomy should not be attended with much more risk than ordinary amputation.

Dr. Whitla (Secretary) did not approve of the theory of tapping a few days before the operation to lessen the shock. He narrated three instances where serious collapse followed tapping, and on the third day alarming inflammatory symptoms supervened from incomplete evacuation of the contents of the cyst. Many such cases were published by Mr. Wheelhouse of Leeds, and he thought it unjustifiable to subject the patient to a double risk, but he approved of tapping as an aid to diagnosis.

New Mode of Operating for Trichiasis and Entropion. By Dr. M'KEOWN.

DR. M'KEOWN stated that the observations he was about to make were meant to refer only to those forms of trichiasis and entropion which resulted from cicatricial changes in the conjunctiva, so common in granular ophthalmia. He commented on various operations—the operation of scalping, or, in other words, cutting off the margin of the lid with the roots of the hairs, he considered to be now no longer a legitimate operation, as we had a choice of several methods which were conservative and not destructive. The destruction of odd hairs by caustic or cautery should not be thought of, as, in the contraction of the tissues which ensued, other hairs would probably be caused to deviate. The endeavour to correct the intertwining of the hairs on the margins of the lids by removing a piece of the skin of the lid, so that the contracted skin would act as a counteracting force to the contraction of the conjunctiva, was generally most unfortunate in the end. The contraction taking place on both the outside and inside of the lid usually brought about a diminution in the depth of the lid, and when this took place to any considerable extent, the lid ceased to be a sufficient cover and protection for the eye during sleep. It was usually only by a great loss of skin that sufficient " tilting" forwards and outwards of the margin of the lid could be secured. He referred particularly to Arlt Jaesche's operation, which consisted in detaching a very narrow strip of the outer margin, containing the cilia, leaving only a narrow attachment at the inner and outer canthus, and then transplanting this band to a higher level on the lid, the way being prepared for this change of position by the removal of a semilunar fold of skin adjacent. It was well known that this operation was very satisfactory in the great majority of cases; but it was acknowledged to have some drawbacks—viz., 1st. The liability of the narrow band to slough, in consequence of its two very slender connexions with the rest of the lid; 2nd. The loss of skin; 3rd. The very protracted and painful character of the operation. Dr. M'Keown then demonstrated a mode of operation free from all these objections, and which consisted in the fact of gliding the whole skin of the lid with the hairs upwards, whilst the tarsal cartilage and conjunctiva were pulled downwards. In Arlt's operation there is, on the contrary, the transplantation of only a narrow band. The operation is performed as follows:—The lid being supported and made tense, by a flat horn protector, introduced between it and the eye-ball, the operator enters Graefe's narrow knife at one extremity of the lid, at the margin between the cilia and Meibomian ducts, and pushes the knife vertically into the lid as far as he can conveniently, and then cuts along to the other extremity of the lid, still following the line between the cilia and the Meibomian ducts. The point of the knife must not be made to emerge through the skin. In this way the lid is com-

pletely split, and the two layers will glide freely on each other. The next step of the operation consists in passing through the tarsal cartilage, which is contained in the conjunctival layer, three strong threads, one at the middle point, and the two others near the outer and inner extremity, respectively, at the distance of about a line or a line and a half from the margin. By means of these threads the cartilage is pulled down towards the cheek, and, when sufficient traction has been made, the threads are to be attached to the cheek securely by adhesive plaster. After the first strip of plaster is placed over the threads, the ends are to be turned up over the plaster and another strip superposed. In this way the threads will not slip. A strong thread is then passed through the outer or cutaneous layer, not far from the cilia, its point of entry and exit being both in the skin, and at equal distance from the hairs. By this thread the skin layer, with the hairs, is drawn up, and then the thread is secured to the forehead by plaster. A little nicety is required in so inserting this thread and making traction that the hairs may be tilted outwards and upwards, and raised sufficiently, but a knowledge of this can only be attained by experience and a careful observation of each operation. The degree of separation between the margins of the outer and inner layer can be regulated at the will of the operator. It is well to have a separation to the extent of two lines at the least. As regards the thread passing through the skin layer, it is advisable not to allow it to remain more than two or three days, whilst those passing through the tarsal cartilage may remain several days longer. The insertion of the threads is an essential in this operation. If not done, the elevator of the lid, which still remains attached to the tarsal cartilage, would, in all probability, draw the cartilage up inside the skin layer, so that the hairs would be just as badly placed as before. The operation may be modified in various ways. For instance, in thickening of the cartilage and deformity, the cartilage may be grooved, pared, or otherwise dealt with, as Streatfield and others have proposed. Dr. M'Keown concluded by stating that he frequently operated in the manner described, and always with the best results. The operation might be performed in a few minutes, whilst for Arlt Jaesche's half an hour or upwards was required. No skin was removed, and, consequently, there was no scar, and no tightening or contraction of the lid. Sloughing was not to be dreaded as in Arlt Jaesche's operation, as both the cutaneous and inner layer had most extensive vascular connexions with the adjacent parts. And, finally, when the healing process is completed, it would require a practised eye to detect that anything had been done.

Dr. Murney said he had operated by the older methods, especially by that of Arlt. The operation now described was entirely new to him. He approved of the principle.

Dr. M. Moore had been present at some of the cases while Dr. M·Keown operated. He was glad to hear of their success, and thought the operation a great improvement. It was simple, expeditious, and highly satisfactory; and, were he called to operate himself, he would decidedly follow Dr. M'Keown's plan.

Dr. J. W. Browne had considerable experience in operating for trichiasis and entropion, which he did by the older methods. As regarded the operation described, he had seen Dr. Perceval Wright operate by a method which seemed to him very similar to that of Dr. M'Keown, about nine years ago.

Dr. Wales was pleased with the ingenuity of the operation. He thought it was a decided improvement upon the older methods.

Dr. Wheeler commented upon its principle, which he thought was a sound one, and must be followed with a better result than the more elaborate and complicated operations.

Dr. Ross (the President) endorsed the remarks of the members present. To him the operation seemed a decided sucess. He reviewed the older operations, and agreed with Dr. M'Keown.

Dr. Esler said that, having had the advantage of being a former pupil at the Eye, Ear, and Throat Hospital, he had seen Dr. M·Keown's method of operation carried into practice, and could bear testimony to the fact that it was expeditious in the performance and highly successful in its results.

A NEW VARIETY OF TUMOUR—DECIDUOMA.

To the already extensive list of tumours we have, according to R. Maier, to add yet another novel histological variety—viz., one having the structure of the *decidua*. According to the observer named, his investigations being founded upon a minute examination of the particulars in two cases, the following are among the principal specialities of the tumours in question :—They only occur in connexion with the womb; their development is influenced by pregnancy and by menstruation ; and they may become a cause of abortion. They vary in shape, being sometimes spheroidal, and at others flattened. Histologically, they are remarkably non-vascular ; and have an areolar structure, in the areolar of which is present a sparing number of characteristic cells.—*Arch. f. Path. Anat.*, Vol. LXXVII.—*Rev. d. Sc. Med.*, 1876.

T. E. L.

CLINICAL RECORDS.

Select Clinical Reports. By RICHARD RYAN, M.D.; Medical Officer to the Bailieborough Union Hospitals.

THE induction of premature labour for the purpose of saving infant life is an operation of sufficient rarity and importance to be placed before the readers of THE DUBLIN JOURNAL OF MEDICAL SCIENCE. It is not strictly accurate to speak of it as a case of induced labour, for though I advised her to come into hospital in order to induce premature labour, and ultimately had recourse to an obstetric operation, the parturient process commenced without any medical or surgical interference. It seems a good instance of the effects of mental concentration on a particular organic function, causing an organ, otherwise independent of the will, to discharge its peculiar function as if in obedience to a direct voluntary impulse.

Deformed Pelvis ; Premature Labour seven months and a half after last Menstruation ; Infant lived twenty-three hours ; Mother made a good Recovery.—Mrs. M., aged thirty-two years, came into hospital by my advice on the 27th of September last, having completed her last menstrual period on the 18th of the previous April. She is one of twins, the other a boy, still living, has congenital antero-posterior spinal curvature, and is short in the breath. She herself has always enjoyed excellent health, and possesses great muscular development with corresponding powers of endurance. On a side view the lumbar curve forward is seen to be much exaggerated, a circumstance of which she herself is aware, and which she says must be caused by her brother's hump pressing in her loins. Both her lenses are affected with congenital *cataracta centralis*, but her vision for all practical purposes is good. This was her sixth pregnancy, including a miscarriage at the third month, which took place between her second and third labours. She is only five years and a half married, so that her case is no exception to the observation of Denman, that "such women have usually a wonderful aptitude to conceive." She had four children at the full time; all were dead-born. Dr. A. attended her in her first labour and performed craniotomy. Dr. B. who attended her in her second labour found the funis prolapsed and pulseless; he delivered by means of the long forceps, but with much difficulty. He also delivered her third child by means of the long forceps, and with equal difficulty. I was in attendance on her fourth labour in September, 1875. When I then examined her my finger fi

impinged on the sacral promontory, which projecting forward contracted the pelvic brim and changed its axis so as to direct it almost horizontally backward. The sacral promontory so placed, and also apparently enlarged, at first reminded one of the fœtal head. On passing my finger above this I found the membranes ruptured, os fully dilated, and feet presenting; but though there had been strong bearing-down labour for six hours, yet the feet had not entered the pelvic cavity owing to the altered direction of its inlet. I brought down the feet with difficulty, as the overhanging promontory so approximated to the symphysis pubis that the feet following the direction of altered axis horizontally backward were arrested by the sharply-curved sacrum ere the knee-joints, which were yet in the false pelvis, could be bent so as to change the direction of the feet to the axis of the cavity and lower outlet. When the head reached the pelvic inlet its further progress was prevented. Though the pelvic cavity was large enough to permit me to bring down both arms and adjust the head in the positions which I thought likely to facilitate its passage, I could not get it down. I applied the long forceps with ease, but my strongest efforts did not seem to advance the head. After about two hours I delivered by traction with the fingers of my right hand spread from behind over both scapulæ and clavicles, and the index finger of my left hooked on the lower maxilla in front, taking care to use the traction force in the direction of the axis of the pelvic brim. Once the head entered the cavity of the pelvis it was expelled without any effort on my part. As the child was alive about twenty minutes before I succeeded in delivering, I kept up artificial respiration for a quarter of an hour, but no signs of life were manifested, so I desisted. There was a sharp hollow in the situation of the left parietal eminence into which the convexity of a large-sized tablespoon would fit, and which was about an inch deep at its deepest part. This corresponded to the abnormal sacral promontory during the passage of the head through the brim of the pelvis. The child was rather a small one. Judging from the results of these four pregnancies and the examinations which I made before and after delivery, I concluded that she would never have a child alive at the full time, and I advised her to come into hospital at the end of the seventh month of pregnancy should she again become pregnant. As already stated she acted on this advice and came in on the 27th of September.

On examining her after admission I could hear the fœtal heart and feel the fœtal movements. She appeared as large as one at the full time, which I attributed to the sacral promontory throwing the uterus forwards, but I subsequently found that the unusually large quantity of liquor amnii would account for it.

October 3rd.—Six days after admission Dr. Robinson very kindly saw the case with me in order to determine the method of bringing on labour.

We were surprised on examining per vaginam to find the os dilated more than half, and quite relaxed, and the hand protruding, with the membranes wrapped so closely around it, that we at first thought the waters had escaped. On being questioned she said she had pains in her back the previous night, which did not allow her to sleep as well as usual. The fœtal movements were so strong and free that each time we examined there was a different presentation. Ordered an enema, as the rectum was loaded.

4th.—Some little pains occasionally; head presenting; os as before.

5th, 9 a.m.—Same as yesterday; ordered a dose of oil. 6 p.m.—As it was now nearly three days since labour commenced, the os being moderately dilated and fully relaxed, we agreed that the time to attempt delivery had arrived. Accordingly I ruptured the membranes, which I found extremely strong and tough, probably owing to the full period of gestation not having been completed. A large quantity of liquor amnii escaped and the funis prolapsed. The head presented, but by passing two fingers into the uterus I displaced it, and, manipulating the uterine tumour externally with the other hand, I was enabled to seize and bring down both feet. As soon as the child's head reached the pelvic brim it was firmly arrested. Having adjusted the head to the pelvic diameters, I introduced the index finger of the left hand into the child's mouth, and depressed the inferior maxilla, whilst I made considerable traction on both feet. In this way I delivered her with much difficulty of a female child, apparently dead. I proceeded at once to keep up artificial respiration, and aided by bleeding from the navel string, the warm bath, and simultaneous cold affusion on the chest, in about half an hour natural respiration took place regularly. The placenta came away in the usual time. The baby, which presented all the appearances of a seven months' child, swallowed imperfectly, whined a good deal, and discharged a viscid bloody sputum a few times during the ensuing night. It sank gradually and died the next evening, about twenty-three hours after delivery. The mother made a good and rapid convalescence, rising on the fifth, and going home on the tenth day. On my recommendation she has promised to return should she again become pregnant.

Unlike the previous child which I delivered, there was no dint in this child's head. Its death I attribute to the force which I found necessary to use in order to complete the delivery, and which, though easily borne by a child at full time, was too severe on a premature child, yet I do not know how I could avoid using the amount of force employed. The surprising thing about this case is the spontaneous occurrence of premature labour exactly at the time at which she had often been previously informed that it would be induced. She did not get any physical or mental cause for it. This is the first case of pelvic deformity I have met during six years, and among about 350 (three hundred and

fifty) midwifery cases. As it is not usual to send for a doctor in the country except in difficult labours, I must conclude that pelvic deformity is of very rare occurrence among a rural population, though I have not infrequently heard of it as a justification for craniotomy. The induction of premature labour in suitable cases permits those members of our profession who are conscientiously opposed to craniotomy to escape the responsibility of so serious and fatal an operation as the Cæsarean section.

Doctor Steevens' Hospital.—*Case of Malignant Disease of the Stomach, presenting badly marked symptoms during life ; Death from Hæmorrhage into the Stomach without much Hæmatemesis.* Under the care of Dr. Grimshaw.

John C., aged thirty-three years, a labourer, admitted October 16, 1876. The patient had just returned from America, where he had resided for three years. He was of intemperate habits, and had suffered from ague in America; he had been in great want before he left America; he was still subject to ague fits and presented the sallow appearance of an aguish subject. He had been occasionally troubled with attacks of pain in the stomach, and alternate constipation and diarrhœa. He was sent to Dr. Grimshaw by a former pupil, as a case of cirrhosis of the liver. This diagnosis suggested careful examination of the liver, which was not found to be altered from the normal size and position; some pain was experienced on pressure over the right hypochondrium and epigastrium; there was no fluid in the abdominal cavity, but there had been hæmorrhage from the bowels, which frequently recurred; the spleen was not enlarged; the stomach was irritable, but there was not persistent vomiting; there was no dilatation of the stomach.

Treatment somewhat improved the patient's condition, but in about a fortnight it was evident that he was becoming worse; the bowels became more constipated, and, when moved, the discharges were dark from altered blood; sometimes red blood was passed; there was occasional vomiting. On November 19th there was considerable discharge of dark matter from the bowels, and coffee-ground vomiting appeared for the first time. He now complained of great pain, was suddenly seized with collapse, and died on the morning of the 21st of November. At no time could any tumour be felt or any very severe pain be produced by pressure over the stomach.

Post mortem examination, by Dr. Bookey, five hours after death.— Body badly nourished; rigor mortis well marked; skin yellowish. Kidneys—right, capsule adherent, central and pyramidal portions hardened—left, in a state of fatty degeneration; mesenteric glands enlarged;

liver, fatty; gall bladder, distended and presenting two pouches; duo-
denum, distended; colon, contracted; no ulceration of intestines, but
general hyperæmia; stomach distended with large amount of coffee-
ground fluid, and large masses of blood-clot; the whole of the interior
surface of the stomach except the orifices was covered with a cancerous
deposit, thickest on the posterior wall; it was rough on the surface, and
thickened the wall to the extent of half an inch in most places.

Remarks, by DR. GRIMSHAW.—The main features in this case were
that during life the extensive gastric disease was not discovered, although
I mentioned to the class, towards the termination of the case, my sus-
picions that there was malignant disease of the stomach—this opinion
being chiefly grounded on the increasing tenderness on pressure over the
epigastric region. The presence of well-marked liver symptoms, and the
evenness of the deposit on the walls of the stomach, were the manifest
cause of the masking of the extensive disease. The freedom from
disease of the orifices of the stomach evidently mitigated the severity of
the symptoms. The immediate cause of death was evidently the *sudden*
effusion of blood into the cavity of the stomach, as proved by the large
quantity of clot found *post mortem.*

DANGER OF RAW MEAT DIET FOR CHILDREN.

DR. BROCHARD, who has done so much in France for improvement in
the hygienic treatment of infants, in his *Almanach Illustré de la jeune Mère*
cautions mothers against the great abuse of raw meat which now takes
place in France in the treatment of the ailments of infancy; and to
which he attributes the great increase of verminous disease, and especially
of tænia. He acknowledges that it may be of use in some difficult
instances of weaning; but these cases are very rare. He has oppor-
tunities of seeing an immense number of infants suffering from diarrhœa
when weaned prematurely or without any precautions being taken; but
he has never had recourse to raw meat for its relief, finding that weak
broths, mixed with milk and light fecular food, are sufficient. As long
as the child has no teeth, good cow's milk, mixed or alternating with
broth, forms the best diet; and when meat is commenced, it should
always be well cooked. If good milk is not procurable, the Swiss con-
densed milk, diluted with tepid water, will succeed well.—*Révue Méd.,*
February 5, 1877; and *Med. Times and Gazette,* February 24, 1877.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. Moore, M.D., F.K.Q.C.P.

VITAL STATISTICS

*Of Eight Large Towns in Ireland, for Four Weeks ending Saturday,
January 27, 1877.*

Towns	Population in 1871	Births Registered	Deaths Registered	Deaths from Zymotic Diseases							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhœa	
Dublin, -	314,666	730	738	3	18	16	5	6	38	10	30·5
Belfast, -	182,082	536	395	—	24	12	4	34	9	9	28·3
Cork, -	91,965	218	204	—	—	1	1	—	3	7	29·0
Limerick, -	44,209	74	106	—	—	—	—	2	3	2	31·3
Derry, -	30,884	56	42	—	—	—	3	4	—	—	17·8
Waterford, -	30,626	58	30	—	—	—	—	—	—	1	12·5
Galway, -	19,692	27	21	—	—	—	—	—	1	—	14·0
Sligo, -	17,285	26	27	—	—	—	—	—	1	3	20·3

Remarks.

A very high death-rate was experienced in Limerick, Dublin, Cork, and Belfast. In Sligo it was low, and in the remaining towns very low. The death-rate was 20·9 per 1,000 of the population annually in London, 22·1 in Edinburgh, and 25·9 in Glasgow. The corrected rate in Dublin was still as high as 29·1. There were 123 deaths from zymotics in Dublin, compared with 114 and 120 in the two preceding four-week periods. Fever, measles, and scarlatina were the most fatal. In Belfast the deaths from whooping-cough numbered 34 against 17 in the previous period. Measles was also very destructive to life. Indeed, the death-rate from the seven chief zymotics was 6·6 in Belfast against an average of 3·1 in twenty large English towns. The corresponding Dublin death-rate was 4·0 per 1,000 annually. Of the 38 fever-deaths in Dublin, 9 were from typhus, 23 from typhoid, and 6 from simple continued fever. In London small-pox caused 381 deaths against 297 and 210 in the two preceding periods.

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20' *N., Long.* 6° 15' *W., for the Month of January,* 1877.

Mean Height of Barometer, - - -	29·697 inches.
Maximal Height of Barometer (on 22nd at 9 a.m.),	30·481 ,,
Minimal Height of Barometer (on 6th at 4 p.m.), -	28·551 ,,
Mean Dry-bulb Temperature, - - -	43·3°
Mean Wet-bulb Temperature, - - -	41·2°
Mean Dew-point Temperature, - - -	38·7°
Mean Elastic Force (Tension) of Aqueous Vapour,	·235 inch.
Mean Humidity, - - - -	82·5 per cent.
Highest Temperature in Shade (on 16th), -	54·1°
Lowest Temperature in Shade (on 13th), -	30·9°
Lowest Temperature on Grass (Radiation) (on 13th),	26·2°
Mean Amount of Cloud, - - -	64·5 per cent.
Rainfall (on 25 days), - - - -	4·322 inches.
General Direction of Wind, - - -	S.W.

Remarks.

The prominent characteristics of the present winter—high winds, warmth, and wet—continued throughout this month. S.W. winds, however, took the place of the S.E. winds so prevalent in November and December, and the rainfalls, although quite as frequent, were much less heavy than those in December. There was scarcely any frost in Dublin, but the alternations in temperature were sudden, frequent, and extreme for this climate. They coincided with alternations in atmospherical pressure and in the direction of the wind, due to the passage of numerous areas of barometrical depression (cyclonic systems) across the British Islands in a north-easterly direction. From the 20th to the morning of the 23rd a short respite from these disturbances occurred owing to the formation of an area of high barometer (anticyclone) over France, England, and Ireland. On the 3rd a high spring-tide was rendered singularly destructive to property on the E. coast of Ireland by the coincidence of a strong easterly gale and a drenching rainfall. This gale was ushered in by extraordinary differences of temperature in Western Europe. At 8 a.m. on the 3rd the thermometer read *minus* 22° at Hernösand (in Sweden), 20° at Nairn (in Scotland), 54° at Scilly, and 59° at Biarritz—an extreme difference of 71° Fahr.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

ON THE INTERNAL USE OF GLYCERINE ASSOCIATED WITH CINCHONA AND WITH IRON SALTS.

M. A. CATILLON (*Repert. de Pharm.*, June 10, 1876) says that glycerine preserves iodide of iron from the alteration it invariably undergoes by exposure to the air, and M. Vezu takes advantage of this fact in proposing to substitute glycerine for water in the solution (1–2) used in pharmacies for the extemporaneous preparation of the syrup. Hitherto, says the author, no one has, to our knowledge, drawn attention to the remarkable property possessed by glycerine of preventing the action of cinchona bark on iron, and thus of removing the incompatibility of two important agents, which it is so often useful to prescribe together. This property is possessed by glycerine to such an extent that cinchona and the iodide of iron even (perhaps the most susceptible of the iron salts employed in medicine) may be associated without decomposition. It is well known that when iodide of iron is added to the syrup or wine of cinchona the liquid first becomes turbid, and speedily assumes an inky appearance, and there is deposited at the end of some days a blackish powder, which contains the iron as tannate. If the usual liquid be replaced by glycerine, the reaction is not observed, and the two (previously) incompatibles remain mixed without either the limpidity or colour of the cinchona preparation being affected. In addition to this, glycerine exerts on cinchona a solvent power comparable to that of alcohol, and which permits the retention of all its principles. Thus, it dissolves entirely the alcoholic extract, which contains them all, and the complex substance designated resin of cinchona, which contains a notable proportion of them. According to Soubeiran, this resin retains, in combination with the derivatives of cincho-tannic acid, known collectively as insoluble cinchona red, a proportion of alkaloid equal in value to one-fourth its weight of sulphate of quinine. The vehicles employed in the ordinary preparations of cinchona precipitate all this active part of the drug.—*Chemist and Druggist.*

PART I.

ORIGINAL COMMUNICATIONS.

ART. X.—*The Influence of Chemical Constitution on Physiological Activity.*[*] By J. EMERSON REYNOLDS, M.D., F.C.S., M.R.I.A.; Professor of Chemistry, University of Dublin.

INTRODUCTION.

ON the 6th of January, 1868, a very remarkable paper was communicated to the Royal Society of Edinburgh, by Drs. Crum-Brown, and Frazer, " On the Changes produced by direct Chemical Addition on the Physiological Action of certain Poisons." In this paper chemical constitution was shown to be a factor of great importance in determining the physiological action of certain substances, chiefly belonging to the group of alkaloids, and some highly interesting results were obtained. Since the date just mentioned numerous researches have been published by eminent observers on the physiological action of medicinal and other bodies, but the results have usually been discussed from the pure physiologist's point of view rather than from the chemical side. In these lectures I propose to consider physiologically active substances, chiefly from the chemist's standpoint, and while following to some extent on the lines of the research of the distinguished pioneers of this branch of inquiry, I shall venture to draw special attention to some matters which have either been overlooked or passed by with trifling notice.

[*] The substance of two Lectures delivered before the King and Queen's College of Physicians in Ireland, on January 22nd and 29th, 1877, being the first and second of the Annual Scientific Lectures for 1877.

I need scarcely say that it would be impossible to deal exhaustively with the wide subject upon which I have the honour to address the College in the short time at my disposal; but I hope, nevertheless, to satisfy my hearers of the truth of the chief propositions I have to state.

It will, no doubt, be readily granted that all chemical substances introduced into the animal organism during life, and which produce any sensible effect, act either physically or chemically, and that in many cases the same substance exerts a physical *and* a chemical action.

By *physical action* we mean that kind of effect which a body can produce without suffering change of composition, or directly causing any such alteration in bodies with which it happens to come in contact. Thus a saline solution of high specific gravity—a strong solution of Rochelle salt, for example—diffuses, as a rule, more slowly through an animal membrane than a weak solution of the *same* salt. Here the effect is almost purely physical, for the rate of diffusion is dependent on the proportion of saline matter in the solution. It is true that the composition of a body, as well as the strength of the solution and other conditions, influences its rate of diffusion; but the effect of composition is eliminated when, as in the case supposed, we compare strong and weak solutions of the same salt.

By *chemical action* we mean that kind of action which involves change in *composition* of the matter engaged.

As I have already stated, the same body may act both physically and chemically; hence we must always remember that the physiological activity of a body may be due in part to the purely physical action exerted by it.

Our aim in these lectures, however, being to trace a connexion between chemical constitution and physiological activity, we shall leave physical action, and, indeed, local corrosive action also, as much as possible on one side.

Let me now explain some chemical terms which must be used in the course of my remarks, but which may not be familiar in their modern sense to some of those whom I have the honour to address.

When we now speak of the *molecule* of a body, we refer to the physical particle of matter, and this is the analogue of the ultimate particle which was long ago called an "atom" by Dalton. Within this molecule of a compound, and, indeed, of most elements, we are compelled to recognise the presence of two or more smaller

portions of the same or of different kinds of matter. It is to these intra-molecular portions of matter that chemists now restrict the term "atom."

Now, the atoms of elementary matter have not all the same power of uniting with each other. Thus the atom of chlorine cannot combine with more than one atom of hydrogen (as in hydrochloric acid, H Cl), whereas the atom of oxygen can unite with two atoms of hydrogen (as in water, H_2 O), but with no more; while the atom of nitrogen can combine with three atoms of hydrogen (as in ammonia, N H_3), and then with an additional atom ot hydrogen and one of chlorine to form N H_4 Cl, or sal-ammoniac. Finally, the atom of carbon unites with four, but no more, hydrogen atoms.

We can thus arrange the chemical elements in groups according to the number of atoms of hydrogen that they can either combine with or displace from chemical combinations, and the elements are spoken of as monad, diad, triad, or pentad, according to their position in the series.

Atomicities of the Common Elements.

Unmetallic			Metallic
H'. Cl'. Br'. I'. F'.	}	MONADS.	Na'. K'. Li'. Ag'.
O''. S''.	}	DIADS.	Pb'', Hg'', Cu''. Cd''. Zn'', Mg''. Ba'', Sr'', Ca''. Fe'', Ni'', Co''.
N'''. P'''. B'''.	}	TRIADS.	Au'''. Bi'''. As'''. Sb'''.
C^{iv}. Si^{iv}. S^{iv}.	}	TETRADS.	Fe^{iv}, Ni^{iv}, Co^{iv}. Mn^{iv}. Al^{iv}. Pt^{iv}, Sn^{iv}, Pb^{iv}.
N^v. P^v.	}	PENTADS.	As^v, V^v. Sb^v. Bi^v.
S^{vi}.	}	HEXADS.	Cr^{vi}, Mn^{vi}.

This so-called "atomicity" of an element is conveniently indicated by lines radiating from the symbol of the element, thus showing the total number of points of attraction, or "bonds," as

they are often called, and consequently the total combining power of the atom.

The molecules of the elements are supposed to contain two elementary atoms of the same kind of matter in combination with each other. According to this view the molecules of hydrogen and of oxygen may be thus represented:—

$$\text{H—H} \qquad \text{O}\underline{}\text{O}.$$

But this combination of an element with itself is feeble and very easily broken up, according to our present knowledge.

Having premised this much, I may now state that we can broadly subdivide chemical compounds into—

(*a*.) Bodies in which all the "bonds" of all the atoms within the molecule are satisfied and are engaged in holding together the molecular edifice. These are called *saturated* compounds.

(*b*.) Bodies in which the bonds are partially satisfied. These are called *unsaturated* compounds.

The differences between the two classes of compounds will be evident on inspection of the following diagram:—

SATURATED COMPOUNDS.	UNSATURATED COMPOUNDS.
Water, H–O–H	Nitric oxide,
Sal ammoniac,	Ammonia,
Marsh gas,	Olefiant gas,
Carbonic anhydride,	Carbonic oxide,

Change can only take place in the molecules of *saturated* compounds by substitution. Thus the metal potassium can displace hydrogen from the molecule of water and form caustic potash; but we cannot, in any true sense, add anything chemically to water.

Change in unsaturated molecules can take place by addition as well as by substitution. Thus nitric oxide can unite with oxygen with extreme facility to form peroxide of nitrogen; ammonia gas with hydrochloric acid to form sal-ammoniac; olefiant gas with

chlorine to form Dutch liquid; and carbonic oxide with oxygen, in order to produce the saturated compound carbonic anhydride.

The bonds not employed in holding together the elements of which an unsaturated compound is composed are said to become *latent*, owing, as some assert, to mutual satisfaction. This is indicated in the diagram by means of broken lines connecting the bonds.

From the statements already made, it will be easily gathered that the unsaturated compounds are, as a rule, the most energetic in their chemical relations. But I may go further and assert that it is chiefly within the large group of unsaturated compounds that we meet with physiologically active bodies. I shall presently bring forward a good deal of evidence in proof of this assertion; but I propose to show, in addition, that the members of the group of saturated compounds which exert decided physiological action are bodies which either contain one or more atoms within the molecule capable of easily replacement by more energetic elements or groups of elements, or which are readily broken up, under certain conditions, into unsaturated molecules within the animal economy.

I hope thus to be able to satisfy you that the study of the chemical constitution of a body, or of the manner in which the elementary atoms are grouped within the molecule, is of the highest importance if we desire to explain the action of many bodies possessing physiological activity.

In seeking now to prove my case, it will conduce to clearness to state at once that I propose to bring forward evidence in support of the following propositions:—

1st. That bodies which contain the *same elements in the same proportions* can differ materially in physiological activity when compared under equally favourable conditions, and that the observed dissimilarity in action is due to difference in chemical constitution.

2nd. That bodies exist which contain the same elements *in different proportions, and are similar in chemical structure*, but which differ materially in degree of physiological activity, and that the observed alteration in action can be explained on chemical grounds.

3rd. That many bodies which are unlike in composition, but which agree in being either actually or constructively unsaturated, and are alike in certain marked chemical relations, often agree in physiological action.

4th. That the result of chemical addition to unsaturated molecules

is either to destroy or materially to modify their physiological action.

Finally, I hope to show how far a knowledge of chemical principles and of the habits of chemical compounds enables us to trace the probable kind of action exerted within the living animal organism by physiologically active bodies.

<div align="center">FIRST PROPOSITION.</div>

Our first proposition asserts *that bodies which contain the same elements in the same proportions can differ materially in physiological activity when compared under equally favourable conditions, and that the observed dissimilarity in action is due to difference in chemical constitution.*

We are acquainted with a considerable number of compounds which have the same centesimal composition, and amongst these I may mention the group of which acetic and lactic acids and grape sugar are members:—

Acetic acid - - - $C_2 H_4 O_2$
Lactic acid - - - $C_3 H_6 O_3$
Grape sugar - - - $C_6 H_{12} O_6$.

These bodies have formulæ which are multiples by integer numbers of the group $C H_2 O$ (or formic aldehyde). Although these bodies differ in their physiological action, these differences are not very striking; hence I pass at once to the consideration of two cases which seems to have been unaccountably overlooked by physiologists.

There are two volatile liquids known to chemists which contain the same elements in the same proportions by weight, and which have the same number of atoms within each molecule. They have, therefore, the same empirical formula, which is $C_2 H_3 N$. These bodies are isomeric, or, strictly speaking, metameric,[*] and, strange to say, are often produced together in a single reaction. One of these bodies is poisonous; the other does not exhibit any strongly-marked physiological activity.

Both these remarkable compounds have the composition of *cyanide of methyl,* and a mixture of the two bodies was long regarded as that body. It has been shown, however, that the

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[*] Compounds which have the same percentage composition are *isomeric;* but we can recognise at least two classes of isomers—(1) polymeric bodies, or those whose molecular weights are different; (2) metameric bodies, or those whose molecular weights are the same, but which differ in constitution.

mixture referred to contains two volatile liquids having different boiling points. The body which possesses the highest boiling point (77° C.) is the true cyanide of methyl, or *nitrile*, and is a body which does not exhibit extraordinary chemical or physiological activity. The other body boils at 59° C., and is called iso-cyanide of methyl, or the *carbamine;* this compound is very active chemically, and is poisonous.

The vapour of the *pure* nitrile* produces little discomfort when inhaled for a short time, but the vapour of the carbamine quickly produces considerable nausea, headache, and general depression. The carbamine is less easily miscible with water than the nitrile; nevertheless, the former quickly destroys life when a mixture of the body and water is injected into the blood of a dog, whereas a mixture of the same weight of the more soluble nitrile and water produces comparatively trifling effects.

It is evidently sufficient for our present purpose to know that one of these metameric bodies possesses much greater physiological activity than the other, and that the active body is one which is most energetic in its chemical relations. This marked difference in chemical and physiological activity is due to difference in chemical constitution, and the constitutional formula assigned to each body, on purely chemical grounds, sufficiently indicates their recognised difference in constitution.
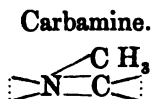
Nitrile.   Carbamine.

In the nitrile the combining power of the carbon atom is represented as wholly satisfied, though two "bonds" of nitrogen seem to be latent. In the carbamine the carbon atom is shown to have but half its "bonds" satisfied. The nitrile cannot be regarded as a saturated compound, because of the latent bonds of nitrogen, and the carbamine is still less so, because of the additional latent bonds of carbon. This difference in degree of chemical saturation of the atoms within the molecule evidently corresponds to the observed difference in physiological activity.

Although I leave these remarkable bodies for the present, as they have served my purpose in proving the truth of my first proposition, I would venture to commend them and their homologues

* There is so much difficulty in obtaining the nitrile free from dangerous impurity that the experiment ought not to be unnecessarily repeated.

to the study of physiologists. If time permitted I could bring forward a large amount of additional evidence bearing upon the same point, but I will now content myself by referring as briefly as possible to the metallic cyanides, as some members of the group are well known to be highly poisonous, while others, which are easily soluble* in water, are inert or nearly so.

A metallic cyanide, or, indeed, hydrocyanic acid itself, may be regarded as the analogue of a carbamine or of a nitrile; thus—

Hydrocyanic acid.　　　　　　　　　Cyanide of potassium.

$$C\equiv N\diagdown^{H}\qquad \text{Nitrile,}\qquad C\equiv N\diagdown^{K}$$

$$N=C\diagup^{H}\qquad \text{Carbamine,}\qquad N=C\diagup^{K}$$
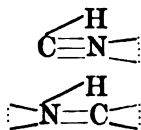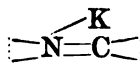
We have no means at present of determining with any degree of precision whether the cyanides of hydrogen or of metals are to be viewed as nitriles or carbamines. If we allowed physiological evidence to materially influence our opinion, we would probably conclude that they resembled the carbamines rather than the nitriles on account of the poisonous activity. It may be that the hydrocyanic acid we are acquainted with is the analogue of the carbamine, and that we may one day succeed in discovering a metameric body (another hydrocyanic acid) which will be almost inert. That day has not yet arrived, however, and all we can say at present is that we know the soluble simple cyanides to be highly poisonous bodies; but we are acquainted with soluble metallic compounds, rich in cyanogen, which are not poisonous; the most remarkable and best known of these is ferrocyanide or yellow prussiate of potassium.

The fatal dose of cyanogen for a man is known to be two grains, if administered in the form of cyanide of potassium or as prussic acid; but five grains of cyanogen can be given to a man in the form of ferrocyanide of potassium without producing any more marked effect than slight diuresis. Ferrocyanide of potassium was at one time regarded as a double cyanide of iron and potassium, but in all true double cyanides the component metals can be recognised with more or less facility by ordinary chemical tests; in the ferrocyanide the iron cannot be detected by ordinary

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* Difference in solubility is often alone sufficient to account for difference of physiological activity ; it is, therefore, of special importance only to compare bodies which can be absorbed with equal or nearly equal facility.

chemical reagents. For this and other reasons we conclude that the ferrocyanide is not a true double cyanide, but rather the salt of a new acid, into the radicle of which the iron has entered. The ordinary formula of the anhydrous compound may be thus written according to each view of its constitution:—

Double Cyanide   -   - $Fe(CN)_2 + 4KCN$.
Ferrocyanide   -   - $K_4\overline{Fe(CN)_6}$.
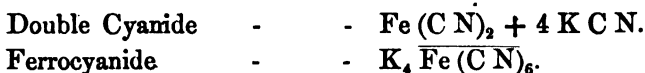
If we take as much cyanide of potassium as contains two grains of cyanogen, and as much ferrous cyanide* as contains one grain of cyanogen, mix the moist precipitate of the latter with the strong aqueous solution of the former, and immediately inject the mixture into the blood of a dog, the animal is almost immediately killed. If, however, we administer in the same way to another dog a quantity of pure ferrocyanide of potassium, likewise containing three grains of cyanogen, the animal survives the dose.

It is at least possible that in the first experiment we have to deal with a true double cyanide, while in the second it is certain that we have to deal with a very differently constituted body.

A true double cyanide of iron and potassium has not yet been obtained in the solid form, but if it exists for a short time in solution, its *unsaturated* cyanogen radicle is doubtless as free to exert its poisonous action as in other double cyanides. In the ferrocyanide, on the other hand, the unsaturated cyanogen groups have united with each other under the influence of the iron to form a compound in which all the atoms are fully engaged in binding together the molecular structure, and which need not be supposed to have within it any latent bonds, if we except the seemingly unimportant case of the iron atom. The developed formula of the ferrocyanide may be then written—
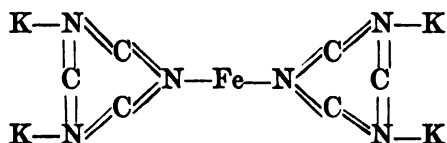
The instances I have now given in evidence of the truth of our first proposition might be largely supplemented, but I apprehend that no one will doubt that the degree of saturation of the com-

* Prepared by adding the calculated quantity of cyanide of potassium solution to ferrous sulphate, collecting and washing the precipitate.

ponents of molecule, and consequently its constitution, materially influence its physiological activity.

We shall now turn to the

SECOND PROPOSITION.

Our second proposition asserts *that groups of bodies exist which contain the same elements in different proportions, and are similar in chemical structure, but which differ materially in degree of physiological activity, and that the observed alteration in action can be explained on chemical grounds.*

At the recent meeting of the British Association, at Glasgow, the results of some highly interesting experiments on the physiological action of the sodium salts of the three kinds of phosphoric acid were communicated by Professor Arthur Gamgee, who, in conjunction with Messrs. Priestly and Larmuth, discovered the existence of remarkable differences in activity between the three compounds. The details of this investigation have not yet been published, but the results obtained, if confirmed by further inquiry, constitute remarkably strong evidence in support of our proposition, though the authors do not seem to be aware of the special importance of their discovery, as I shall presently point out. The researches of Graham first made us distinctly acquainted with the well-marked group of phosphoric acids:—

Ortho-phosphoric acid   -   -   - $H_3 P O_4$
  (Tri-basic.)
Pyro-phosphoric acid   -   -   - $H_4 P_2 O_7$
  (Tetra-basic.)
Meta-phosphoric acid   -   -   - $H P O_3$
  (Mono-basic.)

Corresponding sodic salts are obtainable in which the hydrogen of each acid has been replaced by the equivalent of the alkali metal. Messrs. Gamgee, Priestly, and Larmuth's experiments were conducted with the three sodic phosphates just referred to. Standard solutions of the salts were prepared of such strength that equal volumes contained the same weight of phosphorus. The action of each solution, when injected in the same proportion into the blood of an animal, was carefully noted, and it was found that the sodic ortho-phosphate is almost inert, while the pyro-phosphate is an energetic poison, and the meta-phosphate is also poisonous, but less active than the pyro-phosphate. The authors have shown that

both the poisonous bodies act in nearly the same way on the animal organism. When either salt is injected subcutaneously into the system of a frog, it leads to paralysis of voluntary motion, to arrest of respiration, and soon to stoppage of the heart. The reflex functions of the spinal cord are very soon affected by the poison, but it has no action on the irritability of muscles or nerves.

We have here, then, evidence of a wide difference in degree of physiological activity between the sodic ortho-phosphate and the corresponding salts of the other two phosphoric acids, though all three bodies contain the same elements, and are similar in structure, but contain different proportions of their components. I understood Professor Gamgee to state at the meeting of the British Association that he attributes the poisonous action of the pyro- and meta-phosphates to a possible reduction of either compound in the organism to phosphoretted hydrogen or to elemental phosphorus.[*] The explanation I have now to offer is a very different one, and will be easily understood with the aid of the following diagram:—

SODIC PHOSPHATES.

Ortho-phosphate—

$$\begin{matrix} Na-O\diagdown \\ Na-O-\!\!-P\!\equiv\!\!O \\ Na-O\diagup \end{matrix}$$

Pyro-phoshpate—

$$\begin{matrix} Na-O\diagdown \\ Na-O-\!\!-P\!\equiv\!\!O \\ \quad O\diagvertdown \\ Na-O-\!\!-P\!\equiv\!\!O \\ Na-O\diagup \end{matrix} \; + R'_2O = 2 \left[\begin{matrix} Na-O\diagdown \\ Na-O-\!\!-P\!\equiv\!\!O \\ R\ -O\diagup \end{matrix} \right]$$

Meta-phosphate—

$$\begin{matrix} Na-O\diagdown \\ \quad\diagup\!-P\!\equiv\!\!O \\ O\diagup \end{matrix} \; + R'_2O = \begin{matrix} Na-O\diagdown \\ R\ -O-\!\!-P\!\equiv\!\!O \\ R\ -O\diagup \end{matrix}$$
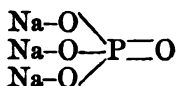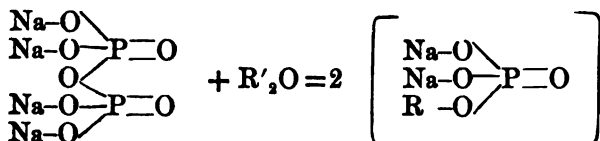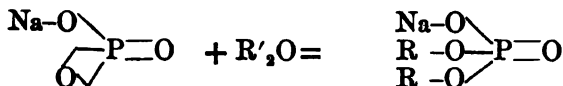
An examination of these developed formulæ will satisfy you that each of the phosphates may be consistently supposed to contain a triad nucleus, consisting of one atom of pentad phosphorus, and one atom of diad oxygen, both "bonds" of which latter are exclusively

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[*] Since the above was written, Professor Gamgee has published a paper on the subject in the Journal of Anatomy and Physiology for January, 1877, in which he expresses opinions more closely in accordance with the view taken by the lecturer.

united with phosphorus. In the ortho-phosphate the three bonds of the nucleus are completely satisfied with the basic radicle, Na'O, or, more generally R'O, when R is some monad metal, or metal-like body. In the pyro-phosphate two only of the three bonds of the nucleus are satisfied by the basic radicle, the third bond being connected with oxygen, which serves to tie it to another phosphoric group. Although all the bonds are saturated in this compound, they seem to be only fully satisfied when united with basic radicles— hence, on treating one molecule of a pyro-phosphate with one molecule of the oxide of any monad metal, two molecules of an ortho-phosphate are formed, as shown in the diagram. In the meta-phosphate but one of the three bonds of the nucleus is satisfied by a basic radicle, the remaining two being saturated by, but apparently not really *satisfied* with, one atom of oxygen, for one molecule of meta-phosphate, when treated with one molecule of an oxide, or a corresponding group, yields one molecule of ortho-phosphate.

As a matter of fact both the pyro- and meta-phosphates are unstable, especially in aqueous solution, and tend to pass into the particularly stable ortho-phosphate by addition of the basic or allied material, provisionally represented in the diagram by R'$_2$ O. According to the general principles already stated, the ortho-phosphate ought to be almost inert, and the other two phosphates physiologically active: à *priori* reasoning would, then, lead us to the conclusion already arrived at by the experimental method in the course of the valuable investigations of Messrs. Gamgee, Priestly, and Larmuth. Assuming now that further inquiry will confirm the opinion of those observers that the pyro-phosphate is more poisonous than the meta-phosphate, we have to account for the difference in activity of the two bodies. This difference is one of degree only and not of kind, as far as we can at present judge. The difference in intra-molecular satisfaction is also one of degree only, and we may reasonably conclude that the pyro- and meta-phosphates retain within the animal organism their tendency to attract and to be attracted by basic material. If, then, their poisonous action is due to the exercise of this power, the question arises whether or not both bodies poison by withdrawing basic material from some important organ. The experimental answer to this is a decided negative, for the pyro-phosphate is most poisonous; yet that body, for a given weight of phosphorus, is capable of withdrawing much less than the meta-phosphate (see diagram). Another view of the matter may be taken, which is in accordance with the results of the

experiments hitherto made. If we suppose that both the pyro- and meta-phosphates are attracted to and accumulated within an important organ by means of an essential basic constituent of that organ with which the phosphates form slightly soluble compounds, we are justified in expecting that the interference of the pyro-phosphate with the performance of the functions of the organ would be much greater than that of the meta-phosphate, because a given weight of basic substance is capable of attracting to a particular point precisely twice the weight of phosphorus in the form of pyro-phosphate as in that of meta-phosphate (see diagram); and the interference with the functions of an organ is, at least, likely to bear some simple and direct proportion to the amount of foreign matter accumulated within or around it. This view is evidently consistent with the fact that the pyro-phosphate is the most poisonous of the two compounds; but, I must add, in concluding this lecture, that the explanation which I have just given in outline, is but one of many which might be suggested—my chief reason for having selected it for remark, in preference to others, being that it is the one which seems to be least open to objection on chemical grounds.

In the next lecture I hope to bring forward some additional evidence in support of the second proposition.

ART. XI.—*On the Embryogeny of the Muscular System.*[*] By ALEXANDER MACALISTER, M.B., Dubl.; Professor of Zoology in the University of Dublin.

THE faculty of motion, that fundamental attribute of all animal bodies, takes place among the simplest living beings by means of the inherent contractility of their constituent protoplasm, the gelatinous albuminoid of which they are composed; but in all animals whose substance consists of differentiated tissues, a special series of contractile cellular elements, the muscular system, is set apart for the purposes of motion.

Why a contractile material should develop in some cells it is not our purpose at present to inquire. I desire rather to direct your attention to some of the stages by which these differentiated elements assume their complexity of adult arrangement in vertebrate animals,

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[*] A Discourse delivered before the King and Queen's College of Physicians in Ireland, on Monday, February 5, 1877, being the third of the Annual Scientific Lectures for 1877.

and to indicate the explanation of certain developmental phenomena of very obscure and singular nature.

I premise that all who wish to follow the inquiry into the development of the muscular system are acquainted with the general features of the embryogeny of the higher animals, and hence I need only remind you *in limine* of the primary stages of the development of the ovum.

Each vertebrate egg is a cell with a nucleus or germinal vesicle, and a nucleolus or germinal spot, and its cell contents consist of two materials, one part being protoplasm, which is directly concerned in the developmental changes, the other being food-yolk, a material to be used chiefly for the nutrition of the developing embryo. On impregnation the yolk segments, dividing into two, then into four, and so on until it has resolved itself into a mulberry-like sphere, composed of minute spherules, which has been called the *morula* stage. According to the amount of the food-yolk present this segmentation is partial or complete, as when there is much of this material that part of the egg which becomes actually organised into the embryo is but small in comparison. This is the case with the egg of the bird, which is hence spoken of as a meroblastic egg in contradistinction to the eggs of mammals, where from the small amount of food-yolk present the whole egg undergoes segmentation, and hence is named holoblastic.

The egg having thus segmented, next becomes converted into a hollow sphere by the liquefaction of the cells of the centre of the morula, and thus it assumes the form of a spherical sac, whose central hollow is known by the name of the segmentation cavity, or cavity of von Baer. When the egg is meroblastic the first cavity which appears is unilateral, and its floor is formed, for the most part, of slightly modified food-yolk spherules; but to its nature we will return.

The wall of the central cavity in the vertebrate embryo appears at a very early stage to be at least bilaminar, and to it the name blastoderm is given. The changes in the nature of the central space we will not touch on now, but will reserve for the next lecture. The outer layer of the blastoderm is the source from whence the epidermis and the central organs of the nervous system are derived, while from the inner originates the mucous membrane of the alimentary canal, with the glandular apparatuses appended thereto. To the outer layer the name epiblast is given; to the inner the name hypoblast.

In the developing eggs of all animals above jelly-fishes and sea anemones, a third lamella appears intermediate between the epiblast and the hypoblast, and to this the name mesoblast is given. From this mesoblast the muscular system arises.

Is this mesoblast a separate, independent structure arising *de novo*, or is it a part of either of the pre-existing two layers, differentiated from them as they are themselves separated by a simple process of growth from the homogeneous protoplasm of the primary egg, and, if so, from which of the two layers is it more especially derived? This is the first disputed question in embryology which our inquiry to-day presents to us, and to understand it fully it is needful for us to trace this middle layer from its first appearance in the animal kingdom. Among jelly-fishes and Hydræ the muscular system is derived exclusively from the epiblast, in the form of those curious contractile processes appended to the outer cell layer, the neuro-muscular cells demonstrated in Hydra by Kleinenberg, and even when a muscular system exists separate from these cells in adult medusæ, the researches of Metschnikoff and Franz Eilhard Schulze show us that it has originated in the same way and from the same layer that the muscular and connective intermediate layer among the cœlenterates arises from the epiblast.

But when we rise higher in the scale of organisation we find that in all higher forms the intermediate lamella has a most remarkable and perfectly diverse origin. In sea urchins and star fishes the ovum at first exhibits to us two layers bounding the central cavity, but from this central hollow tubular outgrowths stretch into the space between the two primary lamellæ, and therein they expand, and as they extend outwards they carry with them on each side as a wall an expanded layer of the hypoblast or lining cells of the primary stomach cavity, and thus there is formed a body cavity external to and surrounding the stomach, and lined by a wall of cells, which at first is directly continuous with the hypoblast. This wall is primarily double, and its outer and inner lamellæ bound between them the body cavity, and finally become the sources of the muscular tissue of the body of these animals. We learn from the stages in the development of several echini that the body cavity is primarily only an outgrowth from the stomach, but so early and so rapidly do these stages succeed each other that in the brittle stars, whose development has been carefully watched by Metschnikoff, the outgrowing processes appear as solid at first, but their expansions secondarily become excavated along what was the

primitive line of cavity. In the sub-kingdom of the worms we find also a similar outgrowing series of tubes from the stomach giving rise to the body cavity, and a similar and even more distinct derivation of the mesoblast from the hypoblast.

In all these the musculature is composed of two layers—one derived from the outer, the other from the inner wall of the expanded tubular offshoots from the stomach sac; and ascending still higher, we learn from the researches of Bobretzsky on the development of Oniscus that in crustacea the mesoblast is similarly derived, while Uljanin, Kowalewsky, and Metschnikoff have shown that the same is true among insects; and Dohrn, Kowalewsky, Rabl, and Bobretzsky prove that it is similar in mollusca. Thus we have the general assent of the leading invertebrate types to the propositions that the body cavity is an offshoot from the stomach, and that the muscular system is derived from its wall.

Turning to the vertebrates, we find that among lampreys, and in that strange aberrant type, the Amphioxus or lancelet, we have clear evidence that the mesoblast has arisen in the same way. And Mr. Balfour's investigations into the development of sharks and rays (by far the most complete and accurate of modern embryological researches) show that in these highly organised though singularly generalised fishes the body cavity arises as two separate lateral spaces, whose walls are united to the wall of the stomach cavity by solid cellular connexions, and which, when compared to the arrangements in the lamprey, bear thereto somewhat of the relation that the brittle star embryos bear to the sea urchin's. In the other higher classes of vertebrata we have the same origin of the body cavity as a pair of lateral cavities, precisely identical in position to the pair of outgrowths, but having lost the primitive tubular connexion; in all other respects, however, perfectly similar.

Thus if, as appears to be the case in cœlenterates, there was among animals a primitive muscular system arising from the epiblast, this has become lost, and its place completely occupied by another muscular system—one purely hypoblastic in origin, and it is to the stages of its development that I now invite your attention.

The embryo of higher vertebrates having shown the formations of its three layers, then develops its medullary groove, which soon closes into the neural canal, along the floor of which the notochord is formed. On each side of this lies the mesoblast, which at a very early period exhibits a separation for the chief part of its extent into two layers, between which a space is apparent—the pleuro-

peritoneal space or the body cavity. This space is bounded by two walls—an outer, which is named the body wall or *somatopleure*, and a visceral wall or *splanchnopleure*, each of which contains a superficial lamina of mesoblast. This cleavage extends as far as the level of the side or top of the neural canal, and it must be recollected that, as I have already indicated, this cleavage is really a developing and modification of the cavity of outgrowth concerned in the first formation of the mesoblast.

The mesoblast does not, however, remain long in this simply cleft condition. Indeed, in many animals this stage is passed by simultaneously with other (morphologically later) changes which mask it completely, for a second division takes place in the mesoblast, and this time the line of division is an antero-posterior one, which divides each half of the mesoblastic area of the body into an axial strip lying on each side of the notochord, and a lateral portion, which comprises the main extent of the mesoblastic constituents of the splanchnopleures and somatopleures, bounding the body cavity.

The axial band of mesoblast, which is itself a partaker in the primitive bilamination, undergoes a still further segmentation, as it is soon cleft into a series of segments arranged in a chain from before backwards, and to these the name *protovertebræ* is given; but as the component mesoblast of the protovertebræ has been a partaker in the primary division into outer and inner lamellæ, this third stage of division cuts up the protovertebral area of the body cavity into a series of tubular spaces, each opening into the body cavity at its ventral end, but blind dorsally. The communication, however, is soon shut up by the development of a cellular mass.

Rapidly are these stages passed through, even in those embryos wherein the successiveness of the divisions have been noticed, so that the slit-like cavities of the protovertebræ and the body cavity in many cases appear to be from the first separate from each other.

A structural differentiation is seen in the protovertebral area at an early stage. Embryonic connective-tissue cells, developed from some of the axial spherical cells, grow inwards, and interpose themselves between the inner part of the protovertebra proper and the sides of the notochord and neural canal. From these are produced the true vertebral body and the axial skeleton, and from the period of their differentiation we will call the outer lamella of the original protovertebra by the name *muscle-plate*. The sympathetic ganglia are produced as developments of the under-surface of the protovertebra, just where they join the intermediate cell-mass.

There are thus in the vertebrate embryo four masses of meso-blast, whose after-history has to be traced. These are—1st, the muscle-plates proper; 2nd, the mesoblast which intervenes between the muscle-plates and the body cavity; this is called by Waldeyer the intermediate cell-mass—that is, the part which fills the region of the primary communication between the body cavity and the primitive protovertebral spaces; 3rd, the mesoblast of the splanch-nopleure; and, 4th, that of the somatopleure. Of these, the first, which shows the foreshadowing of the motor elements to be evolved therefrom, is the protovertebral, for in the embryo dog-fish Mr. Balfour has shown that at a very early stage the innermost lamina becomes differentiated into a layer of muscle whose function is to give the embryo the power of wriggling so as to facilitate respira-tory action. In the frog Bombinator, Goette has shown that the cells of what he calls the nucleus of the protovertebra also become changed, each into a primitive muscle bundle, and the same is the case in another Batrachian Pelobates, according to V. Bambeke.

In the higher vertebrates, such as man, the muscular system consists of six parts :—

1st. The episkeletal muscles of the axis, or the trunk muscles lying on and outside the vertebral column.

2nd. The extrinsic muscles of the limbs, or those joining the limbs to the trunk.

3rd. The intrinsic muscles of the limbs.

4th. The cutaneous muscles, such as *platysma*, and its larger expansion in many animals *panniculus carnosus*.

5th. The hyposkeletal muscles, or those intrinsic trunk muscles which lie beneath the vertebral axis.

6th. The muscles of the visceral systems.

These all differ to some extent in the details of their develop-ment, but all agree in being mesoblastic in origin. We will at present leave out of account the splanchnic muscles, such as those of the alimentary canal and of the heart, and confine ourselves to the five great groups.

The episkeletal spinal muscles, or the intrinsic muscles of the spine, include those which, in human anatomy, we know as the muscles of the third and fourth layers of the back, as well as the intercostals and the oblique muscles of the abdomen. They all take origin directly from the muscle-plate, and as such they at first consist of successive segments corresponding in number to the

protovertebræ, and in this condition they remain in fishes, where the trunk muscles are permanently segmented into myotomes from before backwards, each retaining its embryonic position, as well as its primary nervous supply, from the spinal nerve which arises opposite each protovertebra.

The varying condition, however, of the bony, cartilaginous, or ligamentous connective processes between each of these muscles, as well as the degree of mobility of the trunk in each region, and the conditions of visceral development, cause these to assume a protean variety of disposition in different animals; but yet all the muscles derived from the deeper surface of these plates retain the traces of their segmentation in the frequent and regular connective intersections which remain as traces of their earliest segmented condition.

The neck muscles are derived from the muscle-plates, and here also intersections show themselves, as in the omo- and sterno-hyoids, the adult remains of their embryonic separation in the muscle-plates.

A very singular and interesting alteration of disposition takes place at a very early period in regard to these spinal muscles. At first the connective cells, which are to form the body of the vertebra proper, are segmented in the same planes as the protovertebra, but it is obvious that if the muscles developed from the muscle-plates are designed to move these bones on each other, such an arrangement would be useless; hence a secondary division takes place, replacing the primary, and the vertebral masses of mesoblast become divided into vertebral bodies whose interspaces alternate with the interspaces between the muscle-plates.

Closely connected with this group of muscles we find a second series which join the limb to the trunk, such as the trapezius, latissimus dorsi, rhomboids, &c., as well as the coccygeus, caudo-femoralis, agitator caudæ, &c.; but before studying their method of origin it is necessary that I should premise a few words as to the nature of limbs.

On the third day of incubation of a hen's egg the side of the body wall just along the junction with the splanchnopleure appears raised into a ridge, which runs horizontally backwards from the region of the heart to the level of the allantois. As this ridge lies external to the Wolffian duct, it has been known as the Wolffian ridge, but it is totally unconnected with that body. In structure this ridge consists of a thick, sometimes bilaminar epiblast over a granular mass of the mesoblast of the somatopleure.

In the embryo of the fish Mr. Balfour has found a similar pair of ridges also to come into existence, as well as corresponding ridges along the medio-dorsal and medio-ventral lines; at first precisely similar in appearance to the lateral ridges, but deriving their mesoblast from the layer of uncleft tissue which forms the surface of the membrana reuniens superior of Rathke.

The medio-dorsal fold, instead of remaining continuous, shows a tendency to divide into several parts, each of which becomes, in the adult, a portion of the dorsal fin; its epiblast thickens, and the mesoblast becomes disposed in a row of central columns, the primary fin-rays.

The lateral fold also becomes at an early age divided into three areas; its anterior end elongates and projects, becoming the fore-limb or pectoral fin; its middle part ceases to develop, and its hinder extremity, which grows more slowly than the forepart, by degrees assumes the proportion of a hinder limb; finally the middle ridge disappears, and the two limbs appear as independent lateral outgrowths. In mammals there is a similar lateral ridge on each side, as I have seen in embryo dogs, and thus it is true literally of the origin of limbs that " in continuance they are fashioned."

It would be beside the subject under consideration just now to speak of the origin and relations of the skeletal elements of the limbs, which are thus only specialised parts of a continuous lateral fin. There are, however, two series of factors in each limb, each with a separate muscular apparatus—1st, the limb-girdle; and, 2nd, the limb-ray. The former is probably a modified gill, such as suggested by Anton Dohrn and Gegenbaur, but the evidence is rather suggestive than demonstrative.

The mesoblast of each limb at an early stage differentiates into a stellate-celled axial part, and a columnar-celled peripheral part; the axis elongates and segments into a chain of bones, the supports of the limb, humerus, radius, ulna, &c., while the clothing lamella of elongated cells covers these, and thins off towards the extremity. These covering cells are arranged at first in a single layer, and they early elongate, become contractile, and take on themselves the functions of muscle. The continuous muscular stratum rapidly divides into strips which are to be the separate moving organs of the several bones, and as the successive elements of the limb-axis differentiate from each other, so do these bands of muscle segment off and become grouped around the joints as the superficial and deep strata of muscles of each part.

At first the extremity of the limb, hand or foot, develops, and the intermediate areas of the arm or leg are of later origin.

In studying the formation of the limb-ray in vertebrates, we are led to the conclusion that the mesoblast which forms the limb starts from no constant area of the side of the embryo throughout the vertebrate series, although in the higher specialised forms, like mammals and sauropsids, the origin is usually limited to the tissue adjoining five protovertebræ, as testified by the five supplying spinal nerves and the five digits; but in fishes we find that there are sometimes portions derived from three, four, six, ten, or thirty component segments, shown by the number of radial elements and of spinal nerves, and even in amphibians there may be a sixth digit present, as shown by Leydig. This we know also may occur in man, and indicates the existence of a rudimental fragment from an additional metamere. Some strange teratological instances may be alluded to here, such as the origin of a limb from the medio-dorsal surface of the vertebral column, of which there is a case in the Museum of the Royal College of Surgeons of Ireland—an illustration of an actual persistence and specialisation of the medio-dorsal fin fold.

I cannot leave the subject of limbs without a few words on the mutual relations of the muscles of the fore and hind limbs, on which subject so much has been written, much of which is but the darkening of counsel by words without knowledge—dreary theories yawned into existence by dreamy unphilosophical zootomists. The two limbs, being the fore and hind parts of a primitively continuous lateral fin, consist of primarily homonomous parts, and that there should be a parallelism in their development is just what we might expect, especially as in the primitive forms of animals the two limbs are similar in function, but we cannot expect, and do not find, a closer relationship in detail of organisation between the two limbs than such as results from the development of corresponding though not identical organs of successive segments under nearly parallel conditions, and hence we have no warrant to expect a minute and precise correspondence in detail.

The intrinsic muscles of the limb are thus derivations of the mesoblast of the body wall, outside and ventrad of the muscle-plates, but there is for each limb a group of muscles attached to the limb-girdle joining this structure to the trunk.

The bony axis of the limb-girdles are formed from connective tissue developed on the surface of the muscle-plate, the girdle

being an intercalated structure which displaces the superficial muscular elements derived from the muscle-plates and alters their disposition. Thus the muscles which are attached to the limb-girdle, are the superficial episkeletal muscles, and they primarily form a single sheet composed of fibres concentrating on the zone from the neighbouring protovertebræ, especially from those which correspond to the segments from which the limb-ray arises. In specialised limbs, like the forelimb of man, this extrinsic layer becomes bilaminar, but that is a secondary segmentation, and is of much later origin in the embryo.

Very often these muscles run into the true appendicular muscles, but in such cases wherever a muscle originating from the muscle-plate—that is, an episkeletal muscle—comes in contact with an intrinsic limb-muscle, the two parts are separated by a tendinous inscription. Of this we have an example in the relation of the clavicular trapezius and clavicular deltoid, where an inscription, as in ungulates, or a clavicle (which is, for the most part, an ossified inscription), as in man, intervenes. Of this nature is the line between the dorsi-epitrochlear and the latissimus dorsi and others.

Thus we see the muscles of the limb, taken in general, have a double origin—1st, those of the girdle, which are derived from the muscle-plate, and these are truly episkeletal; and, 2nd, those of the ray, which originate from the somatopleure external to the muscle-plate.

The pectoral group has not been as yet definitely traced in its origin, but seems, as far as I have been able to make out, to originate from the somatopleure anterior to the extremity of the muscle-plate.

The cutaneous muscles form a fourth group, later in development than the episkeletal proper, and arising in the superficial mesoblast of the somatopleure, and are derived from the body wall external to the muscle-plates; hence they are usually lateral or ventral, rarely dorsal in position, and supplied by the anterior terminal twigs of the spinal nerves. There is no truly epidermal, that is, epiblastic, muscular apparatus in any group of animals above the cœlenterates. From some cause as yet unknown to us, such a lamella vanished from the animal kingdom in higher forms, and its substitute is this series, which is only secondarily cutaneous in its relations. Even the muscles of the hair and feather follicles are really mesoblastic derivatives, and by no means primarily connected with the epiblast.

The fifth class consists of the hyposkeletal or hypaxial muscles, called so by Professors Huxley and Stannius, because they are placed beneath the vertebral axis, and differ in many points of structure from the epiaxial. They are usually limited in development to certain areas of the body, and appear to be secondary in point of the time of their formation, and, like all other secondary structures, they are a little obscure in their origin. In man the hypaxial muscles are the *longus colli*, the *recti capitis antici*, *infra-costales*, *sterno-costalis*, *quadratus lumborum psoas*, possibly the *transversalis abdominis*. To these Professor Huxley adds the splanchnic muscles, and others (but without sufficient reason) the pyriformis.

Of the origin of these various accounts have been given; thus Schenk describes them as arising from the inner surface of the protovertebræ. Huxley does not state where they spring from, but says they do not arise in common with the epiblastic muscles, than which they appear much later. Kölliker refers their origin to a small muscle-plate, segmented off from the ventral part of the protovertebra which lies internal to the intermediate cell mass, and which is interrupted in its continuity of development. Goette describes them as arising from the slender inner layer of cells which in amphibians lies on the inside of the nucleus of the protovertebra, while Balfour suggests that they probably arise from the intermediate cell mass of Waldeyer. The difficulty of accurately fixing their origin is great, especially as in many forms, such as fishes, they are unrepresented. As far as I have been able to examine them in dog and sheep-embryos, they seem to spring from the intermediate cell mass, directly above the involution of the Wolffian tubes, and quite separate from the muscle-plates. The mesoblast in which they arise is connected more or less distinctly with the root of the somatopleure, and there is always a close relation in point of position between them and the episkeletal axial muscles of the body wall, although they are thus quite separate. One point is remarkable about these muscles—they are later in development than the episkeletal muscles, not only embryologically in the individual, but ontologically in the series.

Thus far extends our knowledge of the primary origin of the muscular system. We have been able to see that the fourfold mesoblast originates a fourfold musculature—an episkeletal from the muscle-plates, a hyposkeletal from the intermediate cell mass, an appendicular and cutaneous from the somatopleure, and a visceral

from the splanchnopleure. We have also seen that all this com-
plex system of trunk and limb is primarily only the muscular wall
of lateral paired diverticula from the alimentary canal, and that all
the apparent segmentations of the embryo, including the proto-
vertebral, are but returns to the most primitive of embryonic states.
In unravelling these relations embryology has proved itself to be
what it alone can be—the key to unlock the secrets of morphology,
but we have to be content for the present with a simple record of
phenomena, and can only guess vaguely at their direct causation,
for to us it is still as great a mystery as it was to the royal philo-
sopher of old, " How the bones do grow in the womb of her that is
with child."

ART. XII.—*Intra-thoracic Tumour simulating Aneurism; with
Remarks on Differential Diagnosis.*[*] By J. MAGEE FINNY,
M.D., Univ. Dub.; Fellow of the King and Queen's College of
Physicians; Physician to the City of Dublin Hospital.

THE case which I bring under the notice of the Society is one
which has interest connected with it on account of its rarity, the
difficulties attaching to its diagnosis, and the anomalous symptoms
which attended its course. To some of the members of the Society
it may present further interest, as the patient was seen by them
during her life.

It is a case of lympho-sarcomatous tumour, which was both external
to the parietes of the thorax as well as intra-thoracic—involving in
its growth the muscles overlying the sternum and three upper ribs,
as well as the vessels and structures in the anterior mediastinum,
and the sternum itself—and which presented many signs and symp-
toms of aneurism :—

Kate D., aged thirty, servant, presented herself at the extern depart-
ment of the City of Dublin Hospital on October 9th, 1876, complaining
of a swelling in the upper part of the chest, and sought some liniment for
the relief of pain in this place.

My colleague, Dr. Benson, recognised her case to be more serious than
she seemed to think it, and she was admitted the following day under
my care for the purposes of a more thorough investigation.

She is a strong, thick-set, remarkably healthy-looking girl, of very

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* Read before the Medical Society of the King and Queen's College of Physicians,
Wednesday, March 7, 1877.

cheerful and lively disposition, and has always, without exception, enjoyed the best of health. There is no history of any hereditary or specific disease, the several members of her family being all perfectly healthy.

Her account is as follows:—About two years ago she fell down a flight of stairs, coming with much violence against a wooden box at the bottom, over which she rolled. She was seen by one of our leading surgeons, who found no bones were broken and ordered a liniment. The pain in her left shoulder was, however, so transient that she desisted treatment after a few days, continuing in her situation, and thought no more of her accident till about two months and a half ago. She was then seized with an acute pain across the front of the chest, at times extending down the arms. Latterly it has left the arms to radiate round the shoulders to the nape of the neck. A short time after the accession of the pain she noticed an ill-defined prominence in the front of the chest. To this she applied mustard, on two or three occasions, with relief. Latterly she has not slept well, but in every other respect, as to appetite and general health, she states she is perfectly well. In fact for a considerable time after admission she constantly desired to leave hospital to enter a situation. So little did her symptoms impress her that she could not believe she was ill.

The tumour is sufficiently prominent to catch the eye when the chest is exposed; though ill-defined, the centre of the prominence corresponded to the junction of the second left rib with the sternum, being lost in the general contour of the chest about an inch and a half in all directions from this point. It is firm and resisting to the feel, immovable; much handling gives rise to pain. It receives a slight but perceptible impulse; rather abrupt; systolic in time. The impulse was best observed at the sides of the tumour, and here it was distensile in character, but to no marked degree. Over the tumour two sounds, which are evidently the conveyed sounds of the heart, were very audible, the first being accompanied by murmur. They were also readily heard posteriorly between the scapulæ. Over the base of the heart, corresponding to the third left costo-sternal articulation, a murmur is audible, systolic in time, of a blowing character, which is lost towards the apex beat, but is heard upwards in the tumour, and under the left clavicle. It is just as loud mid-sternum as in the tumour. It could also be distinctly heard midway between the left scapula and the spine at the third vertebra; from thence it could be traced down the vessel to the twelfth dorsal vertebra. Except over the seat of the tumour no abnormal dulness could be detected anywhere, and there were no evidences of any pulmonary or pleural engagement, and no enlarged glands could be detected in the neck or axillæ. There was no alteration of the pupils, and though generally dilated they were equally so, and they readily responded to light. She

had neither cough, nor dysphagia, nor alteration of voice. She says she sometimes has flushings of the face and neck followed by perspirations, but this symptom was not observed. Her pulse was quick on admission, and some days after it beat as fast as 140–150 in the minute.

The diagnosis I ventured to make at this time, though guarded, was that we had to deal with a case of tumour springing from, and connected with the manubrium. The possibility of aneurism was entertained, for although the age of the patient excluded it, the fall of so severe a nature might readily have induced some slight break in the wall of the aorta; but the probabilities at this stage of the disease were against it being an aneurism.

The diagnosis of intra-thoracic tumour was based upon—1st. The patient's age and sex. 2nd. The want of proportion between the prominence of the tumour and the amount of impulse. 3rd. The eccentric nature of the pulsation being so small. 4th. The great rapidity of the pulse. 5th. The existence of bruit, not limited, but audible over so much of the thorax. 6th. The position of the tumour being to the left of the sternum.

On the 24th October the tumour had notably increased, and the percussion dulness was of an absolute character over the following area:—Reaching from one inch to the right of the sterno-clavicular joint, on the right side, to one and a-half to the scapular side of the left articulation, it covered the manubrium, and as low as the third rib external to its cartilage on the left side. On deep percussion, dulness could be detected for fully an inch further at the sides, while it extended down to the area of cardiac dulness in the middle. The measurement from the ensiform cartilage to the left sterno-clavicular articulation exceeded that of the right side by a quarter of an inch.

The patient's condition remained for some time much the same. Each day she asked might she soon leave for her situation, and it was only on representing to her that she was seriously ill that she could be induced to remain quiet and in bed. An anodyne liniment, and afterwards two leeches applied over the tumour, gave relief till November 4th, when the pain was so constant and severe as to require the use of a more powerful sedative. She was ordered :—

R.—Extracti opii aquosi, . . gr. 2.
Extracti conii, . . . gr. 6.
Camphoræ, ... gr. 6.
Extracti gentianæ, q.s., ft. pil. vi.
Sig.—One to be taken three times a day.

Nov. 12th.—On paying her an evening visit, I found her lying on her bed in a very peculiar, and, to all appearances, a very uncomfortable posture. She said she had got up, but the pain became so bad that she

threw herself on her bed, and lay in this position for over an hour, fearing to stir lest the pain should return. She said that these sudden attacks of pain came on sometimes when she attempted to lift herself up, or to exercise her arms, as in drawing up the quilt; and yet, that at other times she could do what she liked and be perfectly free from all pain. This fact I had many opportunities of verifying in the after-progress of the case.

As the pulse still continued to be very rapid, 140 in the minute, and she now complained of distressing palpitations, I ordered ten minim doses of tinct. digitalis, and liq. morph., with five m. of tinct. aconitæ in camphor-water, and as the opiate seemed insufficient to dull the pain, the dose of the watery extract was increased to ½ gr. t. d.

When the palpitations were severe, there was a peculiar irregularity of the rhythm of the heart's sounds heard, and this was also at times noticed when all palpitation had ceased. At times a distinct triple sound was audible, due to reduplication of the second sound; but at others, towards the base, a distinct reduplication of both sounds was noticed, of a very remarkable nature. The natural sounds were clearly defined, and with the systolic the pulse coincided, but, instantly after these sounds, two others were heard of unequal length, so that it can best be described by saying the sounds were echoed. This echo had no pulse with it. This sign was not constant, but intermittent, and yet so distinct as to be easily recognised by several others as well as by myself.

The cardiac murmur, which was still very audible over the same region anteriorly as before, was now best heard posteriorly along the left side of the spine. Respiration was more full in the left lung than in the right, but there was no stridor nor tubular sound noticed anywhere.

Nov. 24th (twelve days later).—Very early in the morning her breathing, which was previously quiet, became stridulous and laboured, and on awaking her voice was gone; she was unable to call the nurse, and she found she could not swallow either fluids or solids. When seen at 9 a.m. her countenance was livid, and the supra-sternal region, face and neck, were swollen and greatly œdematous, the clavicles being lost in the swelling, which extended some distance over the pectorals. Her voice had partly returned, but it was completely altered in tone.

Six leeches were at once applied to the upper part of tumour and clavicles, with considerable relief and reduction of the swellings. In the evening four more were applied, two behind each ear. Evening pulse 140; respirations, 26; temperature, 100°.

The next day the swelling of face and neck was less, but the œdema extended to level of the xiphoid cartilage. There is still dyspnœa, which is increased by pressing on the tumour, showing plainly that the tumour is compressing the trachea behind. The thyroid veins and the jugulars seemed those chiefly engaged, as the œdema was confined at this time to

the neck and face. The uvula was swollen, but the epiglottis felt natural. The chest sounds seemed unchanged, except that, in the right lung posteriorly, the breath sounds were more tubular than natural, and at times accompanied by stridor. Anteriorly the respiration was masked by the bruit, which was heard as loudly in the anterior axillary fold as in the subclavicular regions.

The digitalis and aconite were now increased in quantity, and as the urine, which was perfectly healthy, was rather less than normal, spirits of nitre, in decoction of broom tops, was added, while a bolus of pulv. jalap., gr. 30, and hydrarg. subchlorid, gr. 5, was given to relieve the portal circulation.

Nov. 26th.—The dysphagia still continues, but is now confined to solid food. Irritability of the stomach, which set in yesterday, was checked by effervescing draughts.

Nov. 30th.—The dysphagia seems to be passing away, as she can swallow solids with the aid of drinking a little fluid. The swelling of the face and neck is somewhat reduced, and the patient seems to have recovered her usual good spirits; temperature 99·3°; pulse 140. She generally has from one to two paroxysms of intense pain during the twenty-four hours. This the hypodermic injection of ⅛th grain morphia immediately relieves. She now complains of sense of weight, load, and tightness at the upper part of thorax, and at times this is coupled with a dread of movement, lest something should be displaced by change of posture.

No change was noticed for the next ten days, except that she has occasional fits of characteristic laryngeal cough, and her stomach is at times sick; the dysphagia is also present, but liable to variations. These symptoms were, doubtless, due to pressure upon the pneumogastric nerve, while the continued high pulse—for it still, in spite of the treatment, ranges from 130 to 150—may, I conceive, be explained by the function of the inhibitory nerves being arrested, by their being pressed upon by the tumour. I am free to confess that this sudden access of dyspnœa and dysphagia, coupled with the equally rapid variations, shook me in my former diagnosis; and I was induced to agree with the majority of those friends who kindly gave me their counsel and assistance, that it was a case of aneurism rather than any other tumour. Later on I reverted to my first opinion.

During the evening of Dec. 9th there was a sudden increase of the œdema, and the dyspnœa became intense; the brawny hard neck seemed to press in under the jaw and to threaten suffocation. Still there seemed to be no œdema of the glottis, as there was no stridor or aphonia, and to the finger the epiglottis felt thin and natural. I now made a number of incisions with a lancet all round the throat, and the bleeding and serous draining was encouraged by poultices. This gave relief, which

was still further augmented by six leeches behind the mastoids and over the clavicles.

Dec. 10th.—Patient passed a comparatively quiet night, the breathing became easier, and she was able to lie on her side. There was now noticed what seemed a dislocation of the sternal end of the left clavicle upwards; but as she could not endure much handling it could only be a supposition to account for an elevation of one inch in its neighbourhood. To-day the breath sound was much less distinct in the left than the right side, and the same observation refers to the bruit. In fact the bruit was heard now more plainly towards the right axilla than anywhere else. For the first time a new symptom appeared, as streaks of blood, dark in colour, were noticed in the frothy expectoration accompanying the cough. Hæmoptysis lasted for two days and then ceased. The idea of gangrene of the lung was excluded by the absence, during any period of the case, of all fœtidity. Towards evening a return of the urgent dyspnœa and distressing symptoms set in, the face and neck being extremely swollen; they were, however, again relieved by the lancet and leeches. Pain, as if of a constriction, was also complained of low down on the right side, round the edge of the thorax. This was not accompanied by any signs of pleurisy, nor relieved by poultices, and was probably the result of irritation of the phrenic nerve.

Dec. 14th.—Effusion has taken place into the left pleura, as dulness exists over the whole side. This was recognised as hydrothorax by the absence of all signs of inflammation, and of displacement of the heart to the right. There is œdema of left arm and hand. A pustular eruption has appeared on the nose, near the tip. The face and neck are double their natural size, and of a constant dark slaty colour. Strange as it appeared, there was very little enlargement of the superficial veins over the thorax, and none over the abdomen. The patient now, for the first time, became impatient and irritable, and was at times delirious, especially towards evening. This was doubtless due to the great venous stasis in the brain, and probably, in part, to the more frequent injections of morphia, which, though but ¼ grain each, were required oftener to give ease and relieve the sense of suffocation. Having read of the advantages derived from suppositories of iodoform in cancer of the uterus, I determined to administer it by the mouth, at first in 2 grain doses in pill, and afterwards in 5 grain doses in mixture, as the difficulty of swallowing a pill prevented this mode of administration. The prescription I employed was—

R.—Iodoformi, . . . ℥ss.
Spir. æther. nitr., . ℥iij.
Succi Conii, . . ℥ij.
Infusi Juniperi, . . ℥vj.—M.
Sumat. unciam ter in die.

This medicine, with its saffron flavour and smell, was not objected to by the patient, and she herself used to ask for its continuance, as she said it gave her more relief than anything except the morphia injections. It certainly appeared to give relief, as she could breathe with more ease, and could lie on either side; and, on the 23rd, the dysphagia, which had again become extreme, seemed to pass away, as she was able to swallow some pieces of solid food. On the 24th she sat up in a chair at the bedside for a short time, and again on the 26th, the change of posture giving comfort to her. Her spirits again brightened, and she used to say that if only she could breathe easier and the swelling went down, she would be as well as ever, her common expression being—" I am not a bit sick." The œdema, however, steadily increased, and both hands and arms were swollen, and the swelling extended to the labia—the legs being quite free from it.

On Dec. 28th, the patient was much as usual, there having been no new accessions of urgent symptoms since the 14th. She went to sleep quietly under the influence of her usual dose of $\frac{1}{4}$ grain of acetate of morphia, and was seen by my clinical clerk, Mr. Woodroffe, at 2 a.m., in this condition. At 5 a.m., without calling for the nurse, she was observed to lean out of bed to reach a cup of milk, when she suddenly became weak and fell back in bed, and died within five minutes, becoming perfectly pallid.

The *autopsy* was made ten hours after death by myself, with the assistance of our medical resident, Mr. J. F. Woodroffe, to whom I am indebted for the foregoing accurate notes of the case, and for his un-failing attention and kindness to the patient. As the body lay the prominence of the chest was much pronounced. The following measure-ments will give an idea of the swelling:—Round the chest, above mamma and over tumour, 34 inches; and 32 inches at level of the ensi-form cartilage; the neck measured 16 inches. There was swelling and œdema of face, neck, hands, and labia; the legs were quite free.

No varicose veins were visible on thorax or abdomen, the general surface was pale, and over abdomen a fair amount of fat was found. On dissecting off the skin over the thorax it was found almost intimately adherent to the large tumour, hard and firm to the feel, which spread into the pectorals to some extent on both sides, but mostly so on the left, invading all the cellular tissue both above and between the muscles; it also passed into the substance of the left sterno-mastoid, and this it was which produced the fallacious appearance of dislocation of the clavicle upwards.

The muscles lost all their naked-eye appearances, and looked, on recent section, like articular cartilage. The tumour, on section, was white and firm, with very little stroma seen in it, nor was any juice easily pressed out. It was found to extend directly through the sternum, which it

seemed to have either absorbed or to have converted it into its own character, as with a scalpel the bone could be cut through; the upper part of the gladiolus seemed to have softened into a pulpy dark-coloured mass. The new growth also engaged the cartilages of the three left ribs, so that it is not easy to make out a difference between these various parts. The right clavicle for one inch of its inner end is stripped of its periosteum, but both the sterno-clavicular joints are very little affected, the synovial membranes and smooth articular cartilages being intact. This is a point of considerable interest, as bearing upon the subject mentioned by Professor Bennett at the Pathological Society, January, 1877.

On raising the sternum and ribs, the left lung was found retracted to its root, and the lower lobe to be collapsed. It seemed normal on surface, but where it was adherent to the pleura, which lines the mediastinum, the white growth extended into its substance for some short way. There were no signs of pleurisy, but the cavity of the pleura was filled with a clear amber-coloured fluid; a small quantity of the same fluid—evidently passive in its origin—was found in the pericardium. The heart was healthy, all the valves competent, but the lympho-sarcomatous growth had invaded the apex of the pericardium, and had thickened it round the aorta and pulmonary artery, superior vena cava, and upper left pulmonary vein. The aorta was constricted just at the valves, and its wall blended with the malignant growth. This, doubtless, caused the systolic murmur.

The left pulmonary artery is greatly narrowed, and only a probe can be passed into the upper left pulmonary vein. The superior vena cava is free just as it enters the auricle, and here the vena azygos is also pervious, but above this spot it is occluded by the same white matter, and the two innominates are in like condition—while a soft red thrombus plugs the jugular and subclavian. Several large veins communicate with the subclavian, and run anteriorly over the sternum and clavicles.

The whole aorta in its ascending and tranverse portions is surrounded by the mass, which passes up round the left subclavian, carotid, and innominate as far as the top of the sternum.

The mass, passing behind and to the right of the ascending aorta, pressed forwards to the ribs and engaged in like substance the right phrenic nerve and the right pneumogastric—the former is lost in the mass, the latter is swollen, thickened, and intimately adherent to its surface. The recurrent laryngeal is tolerably free. Passing to left it entirely envelops the left pneumogastric and the recurrent; but the posterior pulmonary and some of the œsophageal plexus have, however, escaped involvement.

The left bronchus is incorporated with the tumour, which seems to follow it for a short way, and though its calibre is lessened it is not occluded; the trachea is pushed to the right and backwards, and just

where the œsophagus clears the overlying bronchus, and comes into contact
with the descending thoracic aorta, it is compressed by the tumour, and
its anterior wall blends with it.   This engagement of the tube, doubtless,
caused the dysphagia, owing to its muscular fibres being unable to act
peristaltically.

The back part of the posterior mediastinum escaped, and, as was said,
the vena azygos was quite free.

The aorta, on being laid open behind, shows evidences of endarteritis—
to a slight extent in the descending part, but in the arch itself to an
aggravated degree.

Several lymphatic glands were met in dissection, but although they
lay surrounded by this new growth, they were quite soft and red—in fact,
to all appearances they were healthy, and not larger than might be
expected in any neighbouring irritation—none of them exceeded half
an inch in diameter, the majority met with being less.   On making a
microscopical examination of the tumour, I found it consisted chiefly of
small round cells and lymph-like corpuscles, and this has been confirmed
by Dr. Reuben Harvey, who most kindly examined several portions of
the tumour for me.   He states that it is a good example of lympho-
sarcoma—some cells being almost identical with lymph corpuscles, and
the others being those of small round-celled sarcoma.   No giant cells
were visible—even in the deeper parts next the bone.   As to the latter,
it seemed to be undergoing absorption, several spicula being found
through the overlying portion of the mass.

In my introductory remarks, I stated that cases similar to this
are rare, and that, on account of its rarity, so much interest was
attached to it.

It is not easy to judge of the rarity, or otherwise, of any case
without some reliable data to go upon.   As the most reliable
within my reach, I referred to the " Proceedings of the Patho-
logical Society of Dublin."   At the time I made the research the
exact nature of the tumour was not identified, and I thought it
better to include in my analysis all the cases of cancer of the lungs
and pleura, of mediastinal cancer, and of all other intra-thoracic
tumours (excepting aneurisms).

I find, that since the foundation of the Society in 1838, up to
last year, 1876, there were only twelve cases brought before the
Society.   Of these twelve, but four were mediastinal, and of these
four, three only engaged the intra-pericardial vessels.

From these reported cases my case has a marked difference, as
in not one of them was the tumour both extra and intra-thoracic in
its situation.

Quite recently, in February, Dr. Bennett exhibited a specimen which conformed more to this case than any I have heard or read of. One other case only resembled it in its *post mortem* appearances, which Professor Bennett exhibited in 1864, and which he removed from a female body in the dissecting-room of Trinity College, but it lacked the history and symptomatology to give it much interest.

Before addressing myself to the diagnostic difficulties of the case, there is a physical sign I must allude to, as I never before met with it. It is the reduplication of both sounds of the heart, which was noticeable in November. As I described it, the natural sounds were heard, but, immediately after, two other sounds resembling the first were audible at, and limited to, the base—exactly like an echo.

Dr. Hayden,[a] in his exhaustive treatise, states that while he has met with several cases of reduplication of one or other of the sounds, he met with but two cases of reduplication of both sounds. He does not note whether the pulse was synchronous with both sets of sounds. He rather impresses me with the idea that the second of the sounds is a reduplication of the first, and the fourth of the second. In my case the pulse was only with the first, and the other two were after the pulse. I could not satisfy myself that a second impulse existed, but I rather believe there was no reduplication of the impulse.

For myself I can conceive no explanation of this remarkable phenomenon, except on the theory—and it is but a theory—that both sides of the heart did not act isochronously—the right following the left in its contraction. That such could have existed I see no difficulty, when I see that the nerves, pneumogastric and cardiac, are so involved in the tumour, and that the simultaneousness of influx of the vis nervosa into the two sides of the heart would be prevented, and synchronous contraction consequently precluded. Such an hypothesis has been set forth by Mr. Arch. Bleloch, as quoted by Dr. Hayden.

As is well known to every careful physician, the positive symptoms proper to aneurism of the thoracic arch are but very few, and though at times quite sufficient for diagnosis, at other times in making our diagnosis we have to call into our aid the evidences due to pressure of the tumour upon the neighbouring parts.

The signs and symptoms of aneurism of the arch, when prominent

[a] Diseases of the Heart and Aorta. P. 164. 1875.

enough to reach the chest-wall, are pain, tumour, pulsation, and sounds or murmurs in the region of the vessel. As all these were present in the foregoing case, it may be well to consider *seriatim* how far the similarity existed between such as we expect in aneurism and as they occurred in K. D.

1st. *Pain* was constant, at one time between the shoulders, at others up the neck and behind the sternum; and not only was it constant, but at times was, in addition, neuralgic in its nature—a point to which Dr. Law has called particular attention, as indicative of pressure of an aneurism on some neighbouring bone and causing its absorption.

Although in aneurism all these symptoms be wanting, they are usually more or less present. This pain occurs occasionally in paroxysms, especially on any increase in the arterial blood-pressure by movements, mental emotions, and by pressure from without. Besides the feeling of pulsation in the region of the aneurism, the patient frequently has palpitation of the heart and a feeling of tightness in the chest; certain positions of the body, such as lying on one or other side, originating or increasing their sufferings, while in other positions they disappear.

In connexion with this symptom of pain, a point has been laid much stress upon by some, as serving to aid diagnosis between intra-thoracic cancer and aneurism—I refer to the relief which is obtained in cases of aneurism by the application of leeches. Dr. Stokes was the first to call attention to this diagnostic, and as such it proved a help in a case detailed by Dr. James Little in *The Dublin Quarterly Journal.* In his case, which was at first considered one of aneurism, the want of relief by leeching induced a change of diagnosis in favour of its being cancer. In my case, unfortunately for diagnosis, it was utterly unreliable, as local depletion always gave instant relief, lasting on some occasions for four or five days. It is evident, then, that too much dependence is not to be placed on this fact, and Dr. Risdon Bennett observes " that in several instances he found that much relief has been obtained by the same means (leeching) in intra-thoracic cancer."[a]

2nd. There was *tumour;* its seat was not, indeed, in the usual site of an aneurismal tumour, which is to the right of the sternum about the second and third ribs, as it was situated at the left side and over the second intercostal space. Although this pulmonary area has been termed the " region of romance," and thereby should

[a] Intra-thoracic Growths. P. 117.

awake at once a suspicion that the tumour might not be aneurismal, still many an aneurism has been known to point here, and particularly so when it engages the anterior surface of the transverse position of the arch. That such might have been the case here was further favoured by the area of dulness exceeding the prominence of the tumour, and the shape of the dulness not being triangular, but round like an aneurism.

3rd. There was *pulsation.* When an aneurism has reached the skin by absorption and its contents are fluid, this pulsation is of the distensile or excentric nature, single or double; but when its contents are laminated, this distensile character is lost to a greater or less degree, and the throb of the heart's systole is simple and heaving. In my case, at first the pulsation was distensile to some, though never to any marked degree, and, after œdema set in, it was naturally greatly lessened over the highest part of the prominence; but even then, at the sides of the tumour towards the mammary line, the impulse was very heaving, and beyond what might be expected from a solid tumour overlying, and conveying an impulse from the aorta. I have not seen it mentioned by writers on this subject, nor do I know if the explanation of this sign, as it occurs to me, has been shared in by others, but I believe that to the entire *envelopment of the aorta* in the tumour this strong and violent pulsation may be attributed. Except on this ground or supposition I cannot explain its occurrence, for when the tumour is solid we cannot apply the principle of hydrostatic distension and pulsation which obtains in fluid cysts over an artery, or when, as occurs in extensive empyema of the left side, the impulse of the heart is conveyed through the compressing body of fluid.

4th. The *murmur* which existed in this case was systolic in time, and as it occurred below the seat of dulness and tumour, as well as being heard in and above the tumour, was considered due to some aortic obstruction, and not to be attributed to the tumour. In thoracic aneurism, murmur is rare at all times, and when it exists it is heard in its maximum over the tumour, and not between it and the heart; so, as an aid to diagnosis, its existence here had no weight.

There were two sounds audible in the tumour, but there was no accentuation of the second. This pointed to both aneurismal and malignant tumour, but perhaps most to a new growth.

It will thus be seen that, so far as the signs and symptoms proper to aneurism, the case under consideration might with much colour of probability be set down as one of aneurism.

The nature of the neoplasm tended, I believe, not a little to render the diagnosis of intra-thoracic tumour more difficult, for it lacked certain symptoms which usually are present in, and point to the tumour being cancerous. The first of these which occur to me is the despondency and gloominess of disposition—the lack of all energy of mind and hopelessness of recovery which we see in cases of cancer—a symptom which was totally wanting in my case. This symptom was laid stress on by Dr. Stokes, and referred to as an aid to the diagnosis of cancer by Dr. Hayden.[a]

Other symptoms common to cancer, which my case did not show, were varicosity of the superficial veins, a leaden hue of the skin, and enlarged glands in the neck or axillæ. A distended state of the superficial veins is certainly not a necessary attendant on occlusion of the circulation through the internal veins, occasioned by cancerous disease. Nor is it always absent where that occlusion is produced by an aneurism; but still, where probabilities must be carefully weighed, its absence inclines towards an anuerismal rather than a cancerous cause.

The leaden hue of the skin of cancer was completely wanting in my case, for until the obstruction of the large veins in 'the anterior mediastinum caused venous turgescence and œdema of the face, the patient's appearance was that of a healthy, rather florid country girl. In fact, on entering the ward, one felt inclined to say—there is little serious the matter with this case.

Again, we lost the advantage of, as I have mentioned, enlarged glands, and also of displacement of the heart—although the latter may take place both in cancerous and aneurismal tumour. On the other hand, besides, as I have detailed, wanting many of the *signs* and *symptoms* which generally are present in mediastinal cancer, my case presented *symptoms* which, I confess, were sufficient for a time to stagger the opinion I had formed of the case during the first week or two of her residence in hospital, and which, so far as I was able to judge, pointed to aneurism much more plainly than tumour. Their presence, I presume, aided not a little to produce the impression that the case was one of aneurism, which was so generally expressed by the large majority of my friends, who were so kind as to give me the help of their experience. Of the eight hospital physicians and surgeons who saw and examined this case, two only thought it was not aneurism, one gave no opinion, and five were in favour of aneurism. I mention this fact in no spirit of fault-

[a] Patholog. Soc. Feb. 3rd, 1877.

finding, but to show how extremely difficult was the basis upon which to ground a differential diagnosis.   As Dr. Walshe puts it,* " The diagnosis of a sacculated aneurism of the arch may be, on the one hand, one of the easiest, or, on the other, one of the most difficult problems occurring in the practice of our art—so difficult that certainty proves unattainable, and surmise, more or less strong, alone permissible." The symptoms which shook my confidence in my diagnosis of a tumour springing from the back of the sternum, were the occurrence of seeming displacement of the left clavicle upwards, the sudden aphonia on the night of 14th November, the spasmodic, harsh, loud cough, which had all the metallic ring of irritation of the recurrent laryngeal nerve, which set in on 30th, the hæmoptysis of 10th December, and the dysphagia of both fluids and solids, along with the arching prominence of the chest-wall at the seat of dulness.   All these symptoms were, indeed, it may be said, but the expression of intra-thoracic pressure, and by itself pointed equally to aneurism and cancer; but it was the variability of the aphonia, the dysphagia, the dyspnœa, and the hæmoptysis, which seemed to negative the existence of an intra-thoracic growth. On this point Dr. Stokes[b] says:—" In contrasting the phenomena of pressure in the two classes of intra-thoracic tumours—namely, the aneurismal and the cancerous, we observe certain differences.   In cases of aneurism there is nothing more remarkable than the evidences of a varying pressure. This variation is two fold:—1st, as having reference to a varying amount of pressure on the same part at different times; and, 2nd, as showing a complete and permanent change in the direction of the pressure."
. . . " We find that this character of variation, either in amount or direction of pressure, has not been observed in cancerous tumours, which are either stationary or exhibit a slowly progressive advance. We are not to understand from this that variation is the constant character of aneurism, but merely that its naked occurrence should lead to the diagnosis of aneurismal rather than of cancerous tumour." Dr. Balfour,[c] in speaking of dysphagia as a symptom of aneurism, says:—" The differential diagnosis between this form of dysphagia and that arising from organic stricture is usually readily made, inasmuch as organic dysphagia is unvarying and persistent, while aneurismal dysphagia varies from time to time—is complete

* Diseases of the Heart. 4th edition, p. 500.
[b] Diseases of the Heart and Aorta. 1854. P. 606.
[c] Clinical Lectures on Diseases of the Heart and Aorta. 1876. P. 345.

at one period of the day, and wholly absent at another." These and many other suggestions have been made, and many rules of diagnosis been laid down, to discriminate between intra-thoracic aneurism and intra-thoracic cancer, so that there is little doubt with care a very fair approximate differential diagnosis can be made. But the ground has not been cleared, nor are the means at hand to enable us to discriminate between growths, such as in my case, from aneurism. This difficulty will be much increased when the mediastinal tumour has either commenced in the bone or outside the sternum, and presents the appearance of a tumour involving the costosternal articulations, for then we lose the marked distinction between the progress of an intra-thoracic aneurism and cancer afforded by the tendency of the former to produce absorption of bone and external tumour. That malignant tumours may thus extend from within out there can be no doubt, as such have been recorded, but their numbers are very few. There is no doubt, however, that the possible existence of such a state of things should be borne in mind when the question of differential diagnosis arises between cancer and other tumours *versus* aneurism. Dr. Risdon Bennett holds out, as a probable diagnostic, a peculiarity of febrile disturbance which existed in one case of lymphadenoma that came under his care. The febrile symptoms were exremely paroxysmal, rising at times to 103°; and in a case recorded by Dr. Murchison, which he quotes, similar paroxysms of fever were noticed, each paroxysm being accompanied with an increase of the growth of the tumours and pain.

In my case, as I have stated, there were no such varieties of fever noticeable, as, generally, there was present some slight fever, the temperature varying generally from 99·5° to 101°, and although daily observations were not recorded, it never rose to any great extent.

From a general retrospect of the case, in all its features, as reviewed by the all-important light of *post mortem* revelation, I believe a diagnosis of intra-thoracic tumour, rather than aneurism, might have been not only made but persevered in, on the following grounds:—

1. The age and sex of the patient, for aneurisms are very rare in persons under thirty, and especially so in females.

2. The tumour was large and hard, and the area of percussion dulness very wide, and yet the impulse was not commensurate with either its seeming superficiality or size; the shock was not (as Dr.

Balfour calls particular attention to in cases of aneurism) markedly stronger than that of the heart, although it was quite as strong, and the pulsation was not as distensile as might be expected of an aneurism of such position and dimensions.

3. The sounds of the heart were audible in the tumour (with the first a well-marked bruit), but there was no accentuation of the second sound (Walshe), nor was the bruit of a booming character (Hayden), or confined to the tumour.

4. The œdema of the neck and thorax decidedly preceded that of the arms and hands by many days—the reverse of what would occur in pressure upon the superior vena cava by an aneurism.

5. The constantly rapid pulse—over 130—for two months and a half.

6. The temperature being always sub-febrile.

7. The hydro-thorax, with collapse of the left lung, the result of obstruction to the pulmonary veins.

ART. XIII.—*On the Removal of Foreign Bodies from the Ear.* By ROBERT T. COOPER, M.D., Univ. Dubl.

MR. F. H. WARD, in a letter in the Correspondence section of *The British Medical Journal,* of January 27, 1877, thus expresses his opinion upon the operation of syringing as a means of extracting foreign bodies from the ear:—"I think," says he, "the main point to be observed to obtain success in the operation is to draw the pinna well upwards, in order to alter the relation of the meatus to the unyielding body which it contains, and so give a space for the entrance of the fluid. If this precaution be not observed, or *the stream be caused to impinge directly on the foreign body itself,* one can almost be induced to believe that all the syringing in the world would fail to move the mass in the slightest degree."

We can cordially acquiesce in the idea that causing a stream of water to play directly upon a foreign body, firmly impacted in the external auditory canal, will fail not merely to shift it, but to shift it *in the proper direction,* for this qualification is a very necessary one. And yet it remains an undeniable fact that, however syringing be performed, it has proved itself, if we are to judge by our own experience, and by the expressions of approval with which it is everywhere received, a fairly successful means for removing foreign bodies from the meatus. And this arises, I am forced to believe,

more from the happy natural construction of the auditory canal, as well as from the comparatively small size of the bodies that find their way into it, than from the skilful manner in which the operation of syringing is usually performed.   The operation must necessarily, under any circumstances, fail in such cases as that reported by Mr. Dalby, where some of the cement used in taking a cast of a person's face found its way into the meatus, and there was allowed to set.   " The calibre of the passage is smallest about the middle.   The outer opening is largest from above downwards, but the inner end of the tube is slightly widest in the transverse direction," * so that the seat of impaction of a foreign body is, naturally, where the calibre is least, and therefore midway in the passage.   A jet of water playing upon a foreign body in this situation will have the effect of driving it inwards, and therefore to where the tube is wider from side to side.   There will necessarily, therefore, be room sufficient for the water to insinuate itself between the offending body and the sides of the canal.   In this way a welling-up of fluid will take place behind the foreign body and between it and the tympanic membrane, and which will, in proportion as the water accumulates, exercise pressure outwards upon the body, and in this way lead to its expulsion from the canal.   Very often the bodies most firmly impacted are peas or beads; these being generally round will be found, when driven in, to revolve in the inner extremity of the passage, until the mass of water behind them, being at each stroke of the syringe forcibly agitated, drives them vigorously beyond the narrow portion of the canal.

This, I take it, is the *rationale* of the operation, the membrana tympani being saved—firstly, by the mass of water insinuating itself behind the foreign body and so interposing between it and the membrane; secondly, by the slanting position in which the membrane is thrown across the canal; thirdly, by the tortuosity of the meatus which contributes, in many instances at all events, to break the force of the stream of water; and, fourthly, by the generally unirritating surface of the bodies that find their way into the meatus.

But it is quite within comprehension that were an irregularly-shaped irritating foreign body, such as a cockroach, to find its way into the furthest extremity of the canal, or were there to be impacted in the canal a body large enough to fully occupy its entire

* Quain's Anatomy.   Eighth edition.   Art., " The External Auditory Canal."

inner half, a forcible jet of water playing directly upon this body would have no other effect than to drive it right in upon the furthest extremity of the canal, and in this way cause undue pressure upon the membrana tympani itself, and even if it did not rupture the membrane, at least occasion a great deal of pain by pressing upon the chain of *ossicula auditoria*. Such does not occur, I am prepared to concede, in every case of removal of large quantities of cerumen, bodies that correspond pretty much to this description, but even here it is quite possible that without being in full view of the ceruminous mass upon which we are acting, injury may be done to the delicate membrana tympani. Besides, even in such cases, I know from experience, we can materially expedite matters, and can render our interference much less risky, and much less uncomfortable to the patient, by working with a speculum, and so being enabled to direct the stream of water upon such part of the body as is seen to be giving way.

Again, without actual view of the body, we will often, especially if unpractised operators, undo with one stroke of the piston what we have effected with the immediately preceding ones. And hence, even in the case of impacted cerumen, a much longer time is occupied in removing it than is at all necessary, while with more irritating varieties of foreign bodies absolute danger may be incurred.

Taking into consideration, however, that the shape of the ordinary ear specula, as we find them in the instrument-makers—to wit, Toynbee's, Wilde's, and Gruber's—would be extremely inconvenient for such purpose, being with difficulty held should the syringe-piston be stiff or the syringe itself be too large to manage conveniently with the one hand alone, I have devised an instrument with a handle attached to it, such that while the syringe is being held with the right hand it can be steadied, while the surgeon is syringing, upon the joint of his left thumb flexed upon the handle. In this way we can play steadily upon any part of the foreign body we find to be insecure so as to get the stream of water well behind it, or, if it be necessary from the nature of the impacted mass, as sometimes happens with cerumen firmly clinging to the sides of the canal, we can concentrate our efforts upon such portion of it as seems from its fixedness to require detaching from the sides of the meatus.

As is well known it sometimes takes a long time—where, for example, the cerumen is unusually hard—to detach it, but generally

speaking if we can loosen the outer portion of the mass, hardened and dry from contact with the external air, the remainder, which is usually soft, having been protected from the external air, comes away easily.   Toynbee[a] insists, and very correctly, " that the ear should be drawn backwards so as to straighten the tube; and if this cannot be effected on account of the left hand of the surgeon being otherwise engaged, the posterior part of the meatus should be pressed backwards by the point of the syringe."   But let any-one try and press backwards the wall of the meatus in the way indicated, and he will find it all but impossible to do so without influencing the direction of the stream of water coming from the syringe.   This is not the case when we use pressure backwards with a speculum, as the orifice of the speculum, completely corres-ponding to the calibre of the auditory canal, allows of the jets of water being directed upon any one point.

Another consideration having reference to the patient's comfort, and a very necessary one, is to avoid wetting their clothes when we are syringing.   Toynbee guards against this misadventure by means of his " ear-spout."   " The use of an ear-spout is very serviceable during the process of syringing.   It consists of a spring to pass over the head, at one end of which is a funnel to fit under the ear, down which the water can run into the basin." He then gives us a plate of his well-known and eminently useful " ear-spout " fitted on the head.[b]   The objections I find to Toyn-bee's ear-spout are, that if the spring is not strong enough to cause it to remain sufficiently close under the ear, some drops will now and then find their way down the patient's neck, and so defeat

[a] Diseases of the Ear, p. 52. London. 1868.
[b] *Loc cit*, p. 53.

the object for which the spout was intended; while another, and the principal objection I urge to it, is its constituting an unnecessary addition to the requirements of the surgery—one at least that, as we shall show, may very well be dispensed with. Others use a specially constructed basin (Wilde), others an ordinary drinking tumbler half filled with the fluid to be injected, and direct the patient to press it well in under the ear; but this is objectionable, as a nervous patient, instead of pressing it tightly in under the lobule of his ear, very often will jerk it from him the moment the operation of syringing begins, and so allow space for the stream of water to flow down his neck.

For these reasons the speculum, of which we give a woodcut (*vide* p. 346), is constructed with a spout-handle, an appendage that perfectly answers its two-fold requirements of a handle and a spout. We have termed this a spout-speculum, and feel convinced that any one who once uses it will not care to syringe after the old fashion, and, besides this, it has the not inconsiderable advantage, in these days of unnecessary costly instruments, of being as inexpensive as any ordinary ear speculum, and quite as useful as any of them for the purposes of an optical instrument. I may explain that my speculum is constructed in vulcanite; specula of which, as Pollitzer says (at least he is speaking of much the same material, namely, " rubber"), have the advantage "of being much lighter, and are, therefore, borne in the meatus by the patient, without support, far more easily than the many metallic cones, and, moreover, do not occasion the unpleasant cold sensation caused by a polished metal surface. The dark ground of the inner surface favours a clear definition of the illuminated parts far more than does the slight reflection of light from the polished metal, which increases the brightness of view at the centre." [a]

Vulcanite is a material especially adapted for the requirements of a syringing speculum, which at any moment, on a sudden movement being made by the operator, might be forcibly pressed against and, if of metal, might injure the sides of the meatus. My speculum, as will be observed from the woodcut, is bevelled off at the end, so as to spoon away the mass of foreign matter as it is making its exit from the meatus. This, of course, is not absolutely necessary, though it really is advantageous. With a bright artificial light and a laryngeal mirror on the operator's forehead, and

[a] Pollitzer on the Memb. Tympani. Trans. by Mattewson and Newton. New York. 1869.

an india-rubber tube fitting closely over the spout of the speculum, through which will run the out-going stream of water into an adjoining basin, one can syringe for any reasonable time with this instrument, with the greatest possible comfort to oneself and the least possible inconvenience to one's patient.

To turn now to another consideration regarding the removal of foreign bodies from the ear—namely, the relative advantage of the ordinary ear-scoop and the syringe for this purpose.

Mr. Walter Rivington, in *The British Medical Journal*, March 18th, and again in that of December 16th, 1876, expresses his belief that "the ear-scoop should be banished from the surgical *armamentarium*." We agree most cordially with this opinion; the ear-scoop is an awkward, unsatisfactory, and dangerous instrument, but how can we pretend to displace our instruments unless we come forward with efficient substitutes; and this, so far as I am in possession of the facts, has not been done by this very intelligent surgeon.

Those who have followed us thus far will perceive that the principles upon which syringing, as a means for removing foreign bodies from the meatus, are based, are that the column of water from behind will drive the foreign body outwards, and that, therefore, if the water cannot insinuate itself between the sides of the meatus and the impacted body, the syringing alone cannot possibly effect its object, and that the hitherto very general favour in which it is held is owing to the difference in shape between the inner extremity compared with the middle region of the auditory canal—so much so that in syringing, as we generally perform it, a foreign body impacted midway in the canal is at the commencement of the operation driven further in, prior to the pressure of the column of water from behind it acting so as to impel it outwards. Now, if these premises be true—and any practical surgeon, not to say aurist, will know that they are so—it follows that our chief aim in syringing ought to be to get a mass of water behind the impacted body, so that by exercising pressure upon it from inside, the force of the water will drive it out along the auditory canal. To accomplish this object effectually, I have suggested the construction of a syringe-nozzle, made somewhat in the shape of an ordinary dressing-case ear-scoop (*vide* woodcut, p. 349), the ribbon-shaped opening of the bore of which is placed *above* and somewhat short of the semi-flattened scoop-like extremity, with the object of directing the stream *over* the scoop-end, so that it may pass at first

between the latter and the wall of the meatus. In this way, the small scoop-like extremity having been insinuated a little way between the foreign body and the upper (generally) wall of the meatus, on forcing in the water with the syringe, the stream passing over the scoop-end of the nozzle will be directed into the fundus of the auditory canal, without at all impinging upon the foreign body, all the while steadied, if need be, by pressure of the scoop upon it. The possible objection that may be urged to
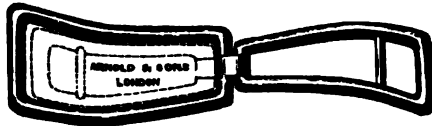
this is that insisted upon by Mr. Rivington, in opposition to the ordinary ear-scoop, in his paper of 16th December, 1876, where he says:—" It occupies a great deal of space, and it must be insinuated by the side of the foreign body, and reach its posterior part, to be of any service at all." In the first place, our scoop does not occupy nearly so much space as an ordinary ear-scoop, and it need not pass further than a very little way beyond the line of impaction; it is enough if it can get sufficiently far in to allow of the water passing to the far side of the foreign body, and to enable us to use gentle pressure from above upon the foreign body, and so prevent its sliding inwards during the act of syringing; for it is not difficult to believe that with the foreign body steadied, and the force of the stream of water directed against the upper wall of the auditory canal, our dependence being upon the force gained by the column of water welling-up from behind, the stream will, without injurious disturbance of the foreign body on the one part, or of the membrana tympani on the other, cause pressure to be made, a true *vis à tergo*, by the column of water pushing from behind as it accumulates against the impacted body; and this—the operator taking care to lift off the pressure from the body by gently directing the nozzle up from off it, when he knows the water has well accumulated—will allow of the water from behind driving the body outwards. But if the body impacted be composed of a hard, non-resisting material, and be firmly wedged in, we cannot, it may be urged, obtain even the small space required for the practical carrying out of these principles. This can hardly be so, for if we give a thought to our anatomy we will find that " the whole cartilage " [of the ear] " may be looked upon as an elongated plate, the lower part of

which is folded round in front so as to bring it *nearly* into contact with the upper part."* So that by taking advantage of this carti-laginous deficiency in the upper wall of the tube, we are enabled, however impacted a body may be, to insert, in certainly the great majority of cases, our scoop-nozzle far enough to allow of the stream of water passing on to the tympanic extremity of the canal. More than this, if the foreign body be irregular in outline, and be cal-culated, by reason of its irregular formation, to do injury to the tympanum, if brought into contact with it, the being able to steady this body by the aid of our syringe-tube, and so to prevent the impacted mass getting further into the canal, is a very obvious advantage—such a body as a cockroach, which formed the subject of discussion between Mr. Rivington and Dr. Alford Nicholls, of Dominica, would be. Certain I am that with our scoop-nozzle and spout-speculum it would be, indeed, an extraordinary mass of cerumen, which would baffle all efforts at dislodgment "for twenty minutes or half an hour," and this "without removing any particles, or even causing the water to be clouded" (Toynbee).

I may add that these instruments are to be had of the well-known surgical instrument makers, Messrs. Arnold and Sons, Smithfield.

METALLOTHERAPY.

At the Société de Biologie, M. Charcot related some interesting experi-ments in the employment of metals. In the case of one hysterical woman, who had lost sensibility of one side, if three or four pieces of gold be applied to the affected side, and kept in their place by a piece of ribbon, at the end of a short time sensibility will return to an extent of from five to eight centimetres above and below the points of applica-tion. Often this sensibility will persist the entire day, and coincident with its return there is an elevation of temperature and an increase of dynamometric force. Sometimes one piece is enough to produce these phenomena. Other metals, such as zinc and copper, have been tried with like results, but some patients are more susceptible of the action of one than of the others. By the same method of procedure similar results were obtained in the case of two persons suffering from loss of sensibility in one side of the body arising from organic cerebral disease, and the sensibility, which was in these two cases restored by the action of the metals, lasted longer than when the malady was only hysterical.

S. W.

* Quain's Anatomy.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Smellie's Treatise on the Theory and Practice of Midwifery. Edited, with Annotations, by ALFRED H. M'CLINTOCK, M.D.; D.C.L., Edin., &c., &c. Vol. I. New Sydenham Society, 1877.

" As a teacher and practitioner, there is no obstetrician—certainly none of the eighteenth century—who deserves so high a place in our estimation as William Smellie. Nay, more, under whichever of these several aspects we may regard him, he scarce has an equal. Whilst of all the men, British and foreign, who have most largely contributed to the advance of sound obstetric knowledge, Smellie may justly stand in the foremost rank. No modern accoucheur, ancient or modern, unfolded so many principles of true obstetric science, and in his practice so consistently acted up to them."

These are the words of our learned editor, and we endorse them; therefore, if any work of our old writers on midwifery may be considered worthy of a reprint, that work is decidedly William Smellie's. Dr. M'Clintock is the man, of all others, we would fix upon as worthy of editing the volume, not only on account of his erudition in this department of medical science, but also because we are of opinion that the labour to him would be one of love. Mr. Jonathan Hutchinson could not have made a more appropriate selection.

The precepts laid down by Smellie in many respects have not been improved upon by the enlightened obstetricians and gynæcologists (as they are pleased to call themselves) of the present day. The work is an emanation from a great mind, endued with a marvellous sagacity, which appeared not to require the aids available to us, moderns, to guide it in pursuing the right track.

In the short space allowed us it would be impossible to give any lengthened review of this first volume of the reprint of Smellie's " Treatise;" suffice it to say that the commentaries of Dr. M'Clintock have brought it up to the present period. This was a desideratum which latterly, as we are informed, occupied the mind

of the late lamented Sir James Y. Simpson, who venerated Smellie even to as great an extent as our friend the editor.

The comments and annotations of the editor exhibit great learning and candour; and the reader will find that due meed of credit has been given to every author since Smellie's day.

In our mind, however, the chief value of this reprint is due to two circumstances—firstly, the republication of Smellie's " Introduction," which is the most masterly review of the " rise and progress " of obstetrics, up to his time, ever printed; a great boon to every pupil in obstetric medicine who aims at becoming more than a mere practitioner. This has been enriched by the editor's annotations, so as to render it a most perfect record. Secondly, the memoir of Smellie by the editor, who, from scanty details scattered far and near, with much labour and with remarkable ability, has produced a biography which, had he written nothing else, would have stamped him as an eminent author.

All the old obstetric classics are to be found upon the shelves of our college libraries, and can be consulted, without inconvenience, by teachers and lecturers. They are volumes not generally required by the ordinary class of students. We, therefore, hope that the New Sydenham Society will stop here, and not inflict upon us any more reprints of them, but permit them to rest in peace.

The present reprint, however, we receive with gratitude, its editor having so enriched it as to make it (so far as the first volume is concerned) almost a handbook of modern midwifery.

A Century of American Medicine. By E. H. CLARKE, H. J. BIGELOW, S. D. GROSS, T. GAILLARD THOMAS, and J. S. BILLINGS. Philadelphia: Henry C. Lea. Pp. 366.

THIS work owes its existence to the Centennial celebrations of last year. The five papers of which it is composed were originally published in *The American Journal of the Medical Sciences*, and are now issued in a more permanent form. They partake largely of the character of a retrospect, with the additional element of patriotism imported into it. We could have wished the pen of so accomplished a surgeon as Bigelow had been employed on some subject of more practical importance than on the conflicting claims of Wells, Jackson, and Morton, to the discovery of anæsthesia. Besides this section, which is entirely devoted to the history of

these three adventurers, the other contributors to the volume have also referred to the matter at length. Overlooking this excusable emphasis on the national claim to the discovery of etherisation, we have read the book with pleasure. We should especially commend the paper on Surgery by Dr. Gross, and that on Gynæcology by Dr. Gaillard Thomas. The latter claims for his countrymen, by the precedence of many years, the successful practice of ovariotomy. So long back as 1809, Ephraim M'Dowell, of Danville, Kentucky, who had for some time been contemplating the possibility of such an operation, found an opportunity, and with a favourable result; again he operated in 1813 and 1816, and the year following he published his cases—all successful. In 1821, Nathan Smith, of Yale College, performed ovariotomy entirely without the knowledge of the fact that he had been preceded by one of his own countrymen. In England ovariotomy was not attempted till 1836. The concluding paper is devoted to American medical literature and institutions. It is written in a fair and candid spirit, and while the writer defends, as well as he can, the medical corporations of his country from the charges of laxness and corruption, he admits that the standard for admission and for graduation at almost all the medical schools is too low, and that at least one half of these schools have no sufficient reason for existence. There are in the cities large numbers of clear-headed, shrewd, well-educated physicians who have the greater part of the valuable practice; but of that higher class of men who love science for its own sake, and whose chief pleasure lies in their original observations, there are very few. Of the highest grade of this class there are no specimens—the John Hunter or Virchow of the United States has not yet given any sign of existence.

Handbook for Attendants on the Insane. By L. S. FORBES WINSLOW, M.B., D.C.L. London: Baillière, Tindall, & Cox. 1877. Pp. 35.

THIS little work contains, in a concise and practical form, the rules and regulations affecting the supervision and care of the insane, as laid down by the English Lunacy Commissioners, together with a synopsis of some of the most important duties with which asylum and private "attendants" must be acquainted. It is of handy size and carefully compiled, and its perusal will, no doubt, prove useful to those to whom it is addressed.

2 A

Notes on Asylums for the Insane in America. By J. C. BUCKNILL,
   M.D., F.R.S.   London: J. & A. Churchill.   1876.   Pp. 88.

DR. BUCKNILL'S " Notes," as now published, consist of a reprint of
a series of papers which appeared from his pen in the columns of
The Lancet for March 18, 1876, and subsequent dates, forming part
of a controversy on the treatment of the insane in the United
States, which originated out of an article in that journal reflecting
unfavourably on American alienist physicians and their institutions
generally, and having reference especially to certain charges which
had been brought against Dr. Nichols, Superintendent of the
National Asylum for the Insane at Philadelphia.   From the careful
and impartial record of his visits to 13 asylums (10 in the Northern
States, and 3 in Lower Canada), besides 2 schools for idiot children,
and 6 institutions for inebriates, Dr. Bucknill shows that many
of the American asylums are most ably managed, and the patients
treated in a manner little different from that of the best of our own
county establishments, and though there may be many more that
are still behindhand, yet it would appear that in a majority of these
instances this is from no defect in the medical officers, but rather
from " reprehensible default in the governing bodies."

   Dr. Bucknill has been accused by some of writing with the in-
tention of upholding the use of mechanical restraint in America,
and in reply to this, in the preface to his " Notes," he takes the
opportunity of repeating his already published opinion on the
subject ("Manual of Psychological Medicine," 3rd Ed., p. 685, *et
seq.*)—namely, that mechanical restraint is an evil and an abuse.
The controversy which led Dr. Bucknill to publish these personal
observations on American asylums has now terminated by the
editor of *The Lancet* stating that he has " neither said nor implied
that there were no good and well managed asylums in the United
States," nor " that he stigmatised the alienists of America as ' mad
doctors' in reproach, and condemned the whole body of psycholo-
gists across the Atlantic for faults in which they do not all
participate."   Dr. Bucknill's faithful record of what he himself saw,
and his outspoken defence of his American brethren at the meeting
of the Medico-Psychological Association in August last—resulting in
an unanimous expression of esteem for the medical men engaged in
the treatment of the insane in America, and of sympathy for those
who had been made the subjects of unfounded accusations and im-
putations—has no doubt contributed much to this end. We believe,

however, that the discussion which has taken place will not be without its fruits, and that as American asylum physicians become more intimately acquainted, through friendly criticism and intercourse, with the system of treatment adopted in these countries, they will be led to reconsider their position in regard to the use of restraint—and, finally, in all probability, to reduce its employment to a minimum, or to abandon it *in toto.*

Dr. Bucknill's "Notes" have been already published in *The American Journal of Insanity* for July and October, 1876, and were, we believe, very favourably received across the Atlantic.

The Irish Medical Directory for the Year 1877. Dublin: Offices of *The Medical Press and Circular.* London: Baillière, Tindall, & Cox. 8vo. Pp. 580.

"THE IRISH MEDICAL DIRECTORY" is yearly winning its way in public estimation, and no medical practitioner's study-table should be without a volume which is now undoubtedly one of the best professional handy-books of the kind.

The editor has evidently spared neither time nor expense in endeavouring to make the Directory as complete as possible; and the increased size of the present volume is caused by the insertion in its pages of the new Army Regulations, the Vivisection Act, the new Militia Medical Regulations, the new Medical Amendment Acts, the Irish Coroners' Act, &c.

In offering our congratulations to those who are responsible for the compilation of *The Irish Medical Directory for* 1877, we have again to deplore the late period at which the volume appears. We trust that the issue for 1878 will be a more timely arrival.

A Handbook of Hygiene and Sanitary Science. By GEORGE WILSON, M.A., M.D. & C.M. (Edin.), F.C.S. Third Edition. London: J. & A. Churchill. 1877. 8vo. Pp. 490.

WE have already expressed a favourable opinion as to the first edition of Dr. Wilson's "Handbook." The work now appears in a much enlarged and decidedly improved form, and may be regarded as a fairly comprehensive treatise on Hygiene and Sanitary Science. As a guide to the Irish Medical Officer of Health, it can never take the place of either of the two volumes recently published in

this country—"The Manual of Public Health for Ireland," and
Dr. Cameron's "Manual of Hygiene." At the same time it is
certainly a book which should be in the possession of every student
of State Medicine.

Medico-Chirurgical Transactions. Second Series. Volume the
　Forty-first. London : Longmans, Green, Reader, & Dyer.
　1876. 8vo, pp. 430.

To this volume of the Medico-Chirurgical Transactions, Dr. W.
Howship Dickinson contributes a valuable paper on the Pathology of
Chorea. His conclusions are based on seven recent autopsies, but
his paper also contains two important tables—one giving particulars
of 22 fatal cases, and the other giving the leading facts in 70 non-
fatal cases, treated in the Hospital for Sick Children.

　In an analysis of the seven cases reported in his communication,
as they ranged in duration from two days to four years, Dr.
Dickinson sums up the pathological changes in the nervous centres
as follows:—

" CASE 1.—Two days. Injection of vessels of all classes in brain and
cord ; most marked in the corpora striata and arbor vitæ, and in the
dorsal region of the cord. Traces of erosion widely distributed. Hæmor-
rhage into, and distension by serum of, the central canal.

" CASE 2.—Twenty-one days. Similar injection of brain, with the
addition of superficial hæmorrhages, and exudation around the arteries of
the corpora striata. Injection of cord, and periarterial erosion in the
dorsal and lumbar regions, marked in the grey matter.

" CASE 3.—Twenty-four days. Injection of the vessels of the brain
of every class, most numerously about the optic thalami ; some extrusion
of corpuscles. Injection of the cord and hæmorrhage into the grey
matter of both dorsal and lumbar regions, symmetrical with regard to
the two sides.

" CASE 4.—Fifty-seven days. Injection of the brain, chiefly venous,
and of the corpora striata. Injection and erosion of the cord, with large
hæmorrhage into the cervical grey matter and smaller elsewhere.

" Case 5.—Sixty-four days. Venous injection of the brain, especially
of the corpora striata, wherein were also periarterial exudations. Arte-
ries in the convolutions near Sylvian fissure surrounded by blood crystals
and *débris*. Injection and scattered erosions of the cord. ' Sclerosis' of
grey matter in both the dorsal and cervical regions placed with bilateral
symmetry.

" CASE 6.—Fatal attack thirteen days. Two precedent attacks (to one of which the older changes were apparently due), the last a year ago. Recent injection, such as in the other cases, of the bodies at floor of the lateral ventricles, and of the cord. Older changes, periarterial degenerations and scattered spots of 'sclerosis' in the *substantia perforata*, and convolution at beginning of the left Sylvian fissure.

" CASE 7.—Four years. Spots of 'sclerosis' numerously set in the substantia perforata, and grey matter of corpora striata symmetrically placed with regard to the two sides. In cord large exudations into grey matter and fissures, chiefly in cervical region."

Thus, the changes throughout the series were remarkably constant in kind and place. In *kind* they were all (allowing that sclerosis is so) directly connected with vascular disturbance. In *place*, the changes affected both brain and cord, being generally—sometimes almost exactly—symmetrical. The nature of the lesions in the brain and cord is not consistent, according to the author, with the somewhat attractive hypothesis of embolism.

There are many other instructive papers in the volume, but we cannot notice them at length. One by Dr. W. Roberts describes a new method, adapted for clinical purposes, of estimating albumen in urine. The principle of the method depends on the observation that when an albuminous urine is progressively diluted with water and tested from time to time with nitric acid, the opacity induced by the acid becomes gradually fainter and fainter until at length it ceases to be visible. This point is reached when the diluted urine contains less than about 0·0014 per cent. of albumen. Dr. A. L. Galabin writes " On the Causation of the Water-hammer Pulse". of aortic regurgitation, and its transformation in different arteries, as illustrated by the graphic method. From this brief notice of a few out of many equally important communications, it will be seen that the " Transactions" fully maintain their high reputation.

ULCERATIVE SYPHILIS.

BISULPHIDE of carbon has recently been recommended by Guillacemet (*Du Traitement des Ulcérations Chroniques, &c.*, Paris, 1876) in the local treatment of ulcerative syphilis. It is said to cause rapid cicatrisation. The following formula possesses the advantages of freedom from the offensive odour which is one of the chief objections to the use of this agent :—℞. Carbon. bisulphid., ℥xvi.; tinct. iodi, ℥iv.; ess. menth. virid., gtt. xvi. M.—*Phil. Med. Times*, Feb. 17, 1877.

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

SAMUEL GORDON, M.D., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, 7th March, 1877.

DR. HENRY KENNEDY, in the Chair.

IN the absence of the author, DR. DUFFEY, Hon. Secretary, read the following paper:—

Notes on Anhidrotics. By T. HAYDEN, F. & V.P., King and Queen's College of Physicians; and Physician to the Mater Misericordiæ Hospital.

IN *The Practitioner* for December, 1876, Dr. Milner Fothergill published a memoir of great practical value on "Anhidrotics," a term by which he appropriately designates medical agents capable of arresting or controlling morbid perspiration. Those of which he specially treats are belladonna and phosphoric acid.

Perspiration may be of the essence of the disease, as in ague and rheumatic fever, and requiring only indirect treatment as a symptom of secondary importance; or it may be incidental, demanding by its excess, and by its unfavourable influence on the issue of the principal disease, direct or even exclusive treatment. Of anhidrotics, as employed in the latter class of cases only, I propose to treat in these brief notes.

Anhidrotics are most frequently demanded in the advanced stages of *pulmonary phthisis*—those, namely, of softening and excavation. In these cases the perspiration alternates with diarrhœa, and is sometimes so profuse as to saturate the night-dress and bedding, leaving the patient in a wretched state of slop and chill, and predisposing to fresh cold with all its evil consequences. It usually occurs during sleep and towards morning, or after a paroxysm of coughing. In the former case it is most effectually

controlled by 5 grs. of Dover's powder, given once or twice in the course of the night; and in the latter, namely, where perspiration is due to excessive coughing, the inhalation of ℳ 10 to 20 of chloroform, or a full dose (ℳ 10 each) of chlorodyne and liquor of morphia, given during the fit of coughing, is the best remedy.

I have occasionally given oxide of zinc in combination with Dover's powder (2½ grs. of each), but the latter given alone is more efficacious, and is likewise useful as a corrective of, or a prophylactic against, diarrhœa.

Tepid sponging of the face, neck, chest, and hands, with toilet vinegar and water in equal proportions at bed-time, is likewise useful as an inhibient of perspiration, and is very agreeable to the patient. The drinks should be cold or tepid; ice may be occasionally sucked, and the night-dress should be put on as warm as it can be borne.

I have used belladonna in a few cases, ½ gr. of the extract given twice in the course of the night, or a full dose (ℳ 30) of the tincture at bed-time. It has in some degree checked the night-sweats of phthisis, but failed to arrest them; and, as an anhidrotic in this disease, it is inferior to Dover's powder.

I have seen profuse and obstinate sweating in the convalescence of *protracted typhoid ;* in the case of a young man, aged twenty-two, it set in on the thirty-sixth day of illness, and resisted for a week all medical treatment; six shirts were saturated every night. Tincture of belladonna and dilute phosphoric acid were given separately and conjointly in ℳ 30 doses, sulphuric and hydrochloric acid; quinine, in 10 gr. doses; Dover's powder; oxide and sulphate of zinc, without effect. At last I induced the patient, much against his will, to get out of bed and remain up for several hours daily; by this means perspiration was promptly and entirely arrested, and a change to the country completed recovery. The bowels were, during convalescence, rather constipated.

For the sweating of *acute miliary tuberculosis* opium is, I think, the best remedy; but any medicine used must be occasionally suspended, else it will lose its effect, a rule which equally applies to the sweating of the more advanced stages of the localised disease.

For the sweating of the hands, feet, and axillæ which occurs to many persons otherwise in good health, Dr. Ringer recommends the liniment of belladonna, rubbed occasionally over the parts affected. I have not tried this, but in the cases just mentioned frequent washing and sponging with the "liquor" obtained from the tan-yards, which is a strong cold infusion of oak-bark, I have found very efficacious.

Perspiration may be checked directly by means of topical astringents or by cold, but these means are of only temporary efficacy, and induce active reaction of the sweat-glands.

Inhibition of blood-supply to these glands through the vaso-motor

nerve-system, as exemplified in Bernard's experiments of galvanising the sympathetic nerve of the sub-maxillary gland, constitutes the scientific plan of treating general hidrosis. The medicinal agents by which this may be accomplished are anhidrotics in the true sense of the word.

It is a question of much interest whether, following the clue afforded by Bernard, physicians may not find in electricity an agent still more potent than medicine in the treatment of hidrosis. I take it that cold liquids introduced into the stomach, being rapidly absorbed, act as anhidrotics by cooling the blood, whilst external heat acts as a peripheral irritant of the vaso-motor centre, whence, by inhibition of the cutaneous vessels, the functional activity of the sweat-glands is restrained or suspended. I hope to publish very soon a summary of cases of hidrosis treated by means of these several agents.

DR. FINNY said he had met with several cases of sweating in phthisis, in hospital practice, and had formerly adopted lines of treatment not mentioned by Dr. Hayden—namely, diluted sulphuric acid, and liq. ferri perchloridi. He had also given three grains of Dover's powder once each night, and had found it occasionally to answer. He had not employed belladonna, but was in the habit of using the sulphate of atropia, mixing half a grain of it with sugar of milk and dividing the mass into forty pills. One of those pills moderated the sweating; and two—viz., one at night and one in the morning—entirely stopped it. At present he had a patient in hospital, in the third stage of phthisis, suffering from perspiration; but he objected to taking these pills lest they would stop the perspiration altogether, as he had an idea that the perspiration was saving his life. He (Dr. Finny) had notes of fifteen other cases in which this remedy had answered. Dr. Finny attested to the value of atropia, when given internally, in the treatment of local sweating, and referred to a case reported by Dr. Grimshaw in *The Irish Hospital Gazette* (1874, p. 52) in which sweating of the feet (bromo-hyperidrosis), produced by wearing India-rubber goloshes made into slippers, had been cured by the administration of atropia. He (Dr. Finny) had found atropia more efficacious in the solid than the liquid form. It had not, however, in his experience, checked the sweating in enteric fever.

DR. MACSWINEY remarked that they were aware that various means of trying to arrest the perspiration of phthisis had been in fashion from time to time. At the commencement of his career he was taught to use diluted mineral acids with bitter infusions. Then acetate of lead was used, apparently with advantage, and enjoyed a high repute. Latterly, belladonna appeared to be almost universally relied on, and, according to his experience, it arrested the perspiration. In a recent case which he had of very distressing hidrosis, half a grain of belladonna was found by

the patient to materially influence the amount of the perspiration. Sponging the body with warm diluted vinegar he had seen to be followed by a very considerable amelioration of the symptoms. But, adverting to the remark of Dr. Finny's patient, he thought it very questionable, after all, whether it was desirable, in every case, to arrest perspiration in phthisis. In the large majority of cases of phthisis, diarrhœa and perspiration alternated; if the one was arrested the other came on. He would go further and say that he did not think it was desirable to use decided and continuous means of totally arresting perspiration in the third stage of phthisis.

The CHAIRMAN said it appeared to him that the desirableness of checking perspiration in phthisis depended on the principle of improving the condition of the system. The extent to which cod liver oil—if the patient could take it—lessened perspiration was astonishing. It had always appeared to him that evening drinks should be checked. The thirst of the patient was often very great; but, by a deprivation of fluids, he had been able to lessen perspiration in phthisis. Again, patients found cotton dresses much pleasanter and better next the skin than linen. Tepid sponging was also very valuable. He agreed with Dr. MacSwiney that perspiration should not be altogether checked even if they had the power of doing so, because that course was very apt to be followed by diarrhœa. Any medicine acting powerfully on the nervous system, such as strychnine, would lessen the perspiration; but it should not be forgotten that those medicines lost their effect when repeated too often. These perspirations were constantly local, being generally confined to the trunk and sometimes even to the head and face.

DR. FINNY said he had never found diarrhœa to come on in consequence of the stoppage of perspiration. He did not believe that the diarrhœa and perspiration were correlative.

DR. FINNY read a paper "On Intra-thoracic Tumour simulating Aneurism." [It will be found at p. 328.]

On the motion of DR. MACSWINEY, the discussion on the paper was adjourned until the next meeting.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-NINTH ANNUAL SESSION.

Saturday, February 10th, 1877.

THOMAS DARBY, F.R.C.S.I., President, in the Chair.

Notes of a Case of Extra-uterine Fœtation. By RICHARD D. PUREFOY, M.B., Dub.

MR. PRESIDENT AND GENTLEMEN,—As cases of extra-uterine fœtation are of infrequent occurrence, and generally present some points of difficulty in diagnosis and treatment, I have ventured to bring before you brief notes of a case which has recently come under my observation. I am indebted to my friend and late colleague, Dr. Macan, for the early history of the patient, which is as follows:—In March, 1873, the patient, a strong, healthy, married woman, who had previously enjoyed good health and regular menstruation, believed herself to be pregnant, as menstruation ceased and abdominal enlargement ensued, which, however, was most marked on the left side. Early in May, and again in June, two severe attacks of pain were experienced, passing down the back of the thigh, in the track of the sciatic nerve. Early in August a third attack of pain took place, and lasted for three weeks, very intense in character, attended with much bearing down, constant yellow discharge per vaginam, frequent micturition, and constipation. During the two months preceding this, marked enlargement of the tumour had taken place, but, at the same time, the patient had lost flesh, and suffered much from thirst and swelling of the feet.

Shortly after this third attack, just described, and about five months from the date of the last menstruation, a vaginal examination was made, when the uterus was found to be very low in the pelvis, the os large, and the cervix somewhat shortened, and in the posterior *cul-de-sac* a firmly fixed tumour, about the size of a child's head. The sound passed into the uterus to the depth of five-and-a-half inches, and could be felt through the abdominal walls above the pubis. The abdomen was swollen, and the left hypogastric region occupied by a hard, movable tumour, which reached an inch above the umbilicus, and seemed to be connected with the uterus, though it lay quite to the left of the sound, when in the uterine cavity. At this time the vaginal discharge had ceased, and the patient complained

only of severe pain in the side, until the following November, when an attack simulating diarrhœa came on, and at the same time fœtal bones, of which I have obtained one, began to escape per vaginam—and this continued for more than a year. At the present date—that is, nearly four years from the occurrence just described—the patient enjoys excellent health, and the only local evidence of what she has undergone is a small cicatrix in the vagina, at the left side of the cervix, and some lateral deflexion of the uterus to the same side. Since observations on the lower animals and on man have demonstrated the presence of spermatozoa on the ovaries and in the whole pelvic portion of the abdominal cavity, we must admit the possibility of extra-uterine fœtation, and considering the numerous circumstances which, singly or combined, may interfere with the normal passage of the ovum into the uterus, we may rather be surprised that cases of it are so rare. For instance, when the serum drains slowly away from a small opening in the Graafian follicle, the ovum may remain behind, and be fecundated there—or the follicle may rupture at a spot distant from the end of the tube, or an abnormal movement of the intestines may drive the ovum from its right course. Again, occlusion of the tube, or destruction of its cilia, may have been caused by catarrh, while an attack of perimetritis is frequently followed by a flexure or constriction of a tube which greatly impedes conception. Clinical observation has shown that a large number of extra-uterine pregnancies occurred in primiparæ, who had lived for some years in sterile marriage, and in many pluriparæ after a long pause in child-bearing.

I propose to make a few brief remarks on the course and termination of the three principal forms of extra-uterine pregnancy—viz., tubal, ovarian, and abdominal, in the order in which I have named them.

Examples of tubal pregnancy occur when the ovum is arrested in the Fallopian tube, from some of the diseased conditions just mentioned, and sometimes from the presence of a polypus at the uterine extremity of the tube, as in Beck's case; or, as in the very rare case mentioned by Häss-finther, an ovum which has normally entered the uterine cavity may pass thence into a Fallopian tube, and there develop.

Coincident with the development of the ovum, changes in the tubal mucous membrane take place, analogous to, or—according to Schrœder—identical with those observed in the uterine mucous membrane in normal gestation, though Playfair denies that any decidua reflexa is formed.

Mr. Holmes, in Vol. XLIII. of "Medico-Chirurgical Transactions," gives an account of a dissection of a recent case of tubal pregnancy, in which he found a third membrane surrounding the ovum, distinct from the chorion and amnion, separable into two layers, and not forming part of the wall of the tube. The distension of the tube, due to the growth of the ovum, generally obliterates both its extremities, particularly that corresponding to the uterus, and its muscular fibres are so forced apart

that the fœtal sac consists only of peritoneum and tubal mucous membrane.

At the same time important changes take place in the uterus. The whole organ hypertrophies, and its mucous membrane is converted into a true decidua. Its occasional absence at *post mortem* examinations is probably due to the fact that it is sometimes thrown off *en masse* during the hæmorrhages which often precede the fatal issue of such cases. The most frequent termination of tubal pregnancy is in rupture of the sac before the fifth month, generally at its thinnest part, but sometimes at the placental insertion ; and death mostly follows, either from profuse internal hæmorrhage at the time, or from the consequent peritonitis. However, after its escape into the abdominal cavity the ovum may be encapsuled, and if at an early period of development it may, to a great extent, be re-absorbed; or, if the embryo be older, it may undergo the changes which will be mentioned when speaking of abdominal pregnancy. Occasionally, as in the cases recorded in *The Lancet* of 1861, and *British Medical Journal* of 1859, tubal pregnancy may be prolonged nearly to full term. Mr. Hutchinson, in 1873, recorded a case of what proved to be Fallopian gestation, prolonged to full term, and simulating ovarian dropsy so closely that the abdominal tumour was punctured, unfortunately with a fatal result. Undoubtedly the most favourable termination of tubal pregnancy is when the embryo perishes early, and recovery ensues without any rupture. Various measures have been taken to secure this end, which will be mentioned when speaking of treatment. With regard to diagnosis, Schrœder has well said that it is generally easy in the first months to make sure of an extra-uterine tumour, while the diagnosis of a pregnancy is frequently involved in difficulty; at a later period pregnancy can easily be diagnosed, but to show that the fœtus is outside the uterus may be very difficult. In the first place it is to be observed that all the usual sympathetic disturbances of pregnancy exist—the breasts enlarge, the areolæ darken, and morning sickness is present. Menstruation is generally arrested, but there are often irregular hæmorrhages, especially before rupture takes place. Barnes attributes these to partial detachment of the chorion villi, produced by the ovum growing out of proportion to the tube in which it is contained, and that they are thus exactly similar in their mode of occurrence to the hæmorrhages which occur in cases of placenta prævia in the early months. If, under these circumstances, a careful bi-manual examination reveals the presence of an elastic, sensitive tumour on one side of the uterus, and that the latter is moderately enlarged, the case is probably one of extra-uterine pregnancy; the use of the uterine sound in very skilful hands may demonstrate that the uterus is empty, and this will also help us. We may be quite certain if hard and movable parts can be felt in the extra-uterine tumour, and if the fluid obtained from it by puncture have the usual characters of liquor

amnii.* When by these various means of examination a satisfactory diagnosis can be arrived at, and the sac is accessible from the vagina, rectum, or abdominal walls, we are justified in puncturing it with a fine trocar, allowing the liquor amnii to drain off, and this can generally be done most readily through the vagina. The fœtus then dies, and, according to the time of its development, is either reabsorbed or converted into a lithopædion. Dr. Bachetti has successfully used the continuous current to obtain the same result. Recently Dr. Thomas, of New York, opened the cyst through the vagina by a platinum knife, heated by a galvano-caustic battery, and, through the opening thus obtained, removed the fœtus. In attempting the removal of the placenta violent hæmorrhage ensued, requiring the use of an astringent injection to stop it ; the patient made a good recovery.

More frequently our aid is sought when rupture has taken place, and our efforts must then be directed to stop the hæmorrhage, a matter of no little difficulty. Ice applied to the abdomen, and, if possible, compression of the abdominal aorta, are the most efficacious means. If assistance be at hand immediately after the rupture, and if the ovum be of large size, perhaps the most successful treatment would be the Cæsarean section in order to stop the hæmorrhage, remove the fœtus, and, if possible, the whole sac which contains it. It is supposed by Bernutz, and his opinion is shared by Barnes, that rupture which does not prove fatal is probably not very rare in the first few days of extrauterine gestation, and that it is not an uncommon cause of certain forms of pelvic hæmatocele. I had recently under my care a young unmarried woman suffering from uterine hæmorrhage, which followed some unusual exertions. The local signs of pelvic hæmatocele were present, and also such mammary development as strongly suggested pregnancy.

A variety of tubal gestation known as tubo-uterine, or interstitial, presents some points of difference from the form just described. Here the ovum develops in a portion of the tube already continuous with the uterine substance. In most such cases rupture occurs during the first three months. After rupture of the tubal mucous membrane the ovum may remain between the separated muscular fibres of the uterus, and gradually stretching the peritoneum, as it enlarges, may continue to develop up to the normal end of pregnancy.

* Dr. Barnes says three conditions may render diagnosis difficult—retroversion of the gravid womb, a small ovarian cyst, and retro-uterine hæmatocele. The first and third of these conditions will commonly cause retention of urine, which is not common in tubal gestation. In the first there is almost always a history of pregnancy and the signs of it. In the third there also may be a history of pregnancy. Retroversion may be distinguished by tracing the rounded body of the uterus by rectal and vaginal touch. In hæmatocele the effusion of blood follows severe symptoms suddenly produced, and the uterus is much less developed than in tubal pregnancy. A small ovarian cyst may cause retention of urine, but seldom interferes with menstruation.

I may illustrate this fact by the following case, recorded by Schultze in 1863 :—A healthy primipara menstruated last in June, 1862. Labour began on March 30, 1863. On the following day she died of convulsions, and the Cæsarean section was performed ten minutes after death. The child was dead, presenting in the first position, and its right foot protruded through an aperture in the right segment of the fundus. The peritoneal insertion of the right tube in the uterus was close to the edge of the uterine rent; and the seat of the rupture showed a defective muscular coat, forming a pouch, as if out of a dilatation of the uterine end of the tube. In the same year Breslau recorded the case of a healthy woman, aged thirty, who six months after marriage was suddenly attacked, without any evident cause, with symptoms of abdominal hæmorrhage, followed by death in about six hours.

Post mortem.—An extensive effusion of blood was found in the abdominal cavity, and at the point of opening of the left tube into the uterus, half belonging to the uterine parenchyma and half to the tube was a ruptured sac, containing coagulated blood, mixed with chorion villi. From the size of the sac the pregnancy could not have reached more than the seventh week. A mucous polypus was found just below the uterine orifice of the tube, and very probably obstructed the passage of the ovum into the uterus.

In the Obstetrical Transactions for the year 1867, Braxton Hicks describes a case of interstitial pregnancy, which for some time enlarged equally towards the peritoneal and uterine cavities, but finally burst into the latter, permitting the escape of the fœtus, and simulating an ordinary abortion ; the sac then compressing, the placenta still retained in it, a large vein just beneath the peritoneum burst and produced fatal hæmorrhage. The difference between interstitial and simple tubal pregnancy can be shown in the dead body by the relation which the round ligament bears to the sac containing the fœtus. In the interstitial form the ligament passes external to the sac, and in simple tubal pregnancy between the sac and the uterus. A distinction between this and gestation in a perfect or rudimentary horn of the uterus offers great difficulties, since the relation of the round ligament to the sac of the fœtus is the same in both, and the thickness of the septum between the sac and uterus proper differs only in degree, though it is generally less in the interstitial form. Ovariotubal pregnancy occurs when the ovary and tube both take part in the formation of the fœtal sac.

Dr. Hayden, of London, in the year 1862, brought before the Obstetrical Society the case of a young woman who died after a few hours illness from rupture of the sac of a tubal gestation ; the *post mortem* examination, by Dr. Smith and Dr. Hicks, showed that some time before the patient had conceived extra-úterine, the fœtus being attached to the fimbriated extremity of the Fallopian tube in such a way as not to prevent

conception, that the fœtus died about the second month of pregnancy, still enclosed in its ovular structures; that at a later period she again conceived extra-uterine, but the ovum now descended to the middle of the tube, where it became arrested, lived about three months, and then died; three months after which the sac burst and death ensued.

A case remarkably similar in many particulars to this was recorded some years ago by Mr. Tufnell. A patient in the fourth month of pregancy was seized after a long walk with violent abdominal pain and prostration, followed by death in twenty-four hours. At the *post mortem* examination the abdomen was found to contain a large quantity of fluid blood, in which a fœtus one inch in length was floating; a rent in the right tube disclosed a cyst from which the fœtus had escaped. In the uterus was found a healthy male fœtus proportionate to the date of conception.

When the fecundated ovum remains and becomes developed in the ovary, we have ovarian pregnancy, but this occurs so rarely that there are few well authenticated cases on record, and the diagnosis of it during life may be quite impossible.

Hecker, in 1859, collected statistics of 222 cases of extra-uterine gestation illustrating the tubal, interstitial, and abdominal varieties, at the same time expressing the opinion that the existence of ovarian pregnancy still wanted proof.

There is, however, in the museum of Olmutz a preparation of a fœtal sac, in the walls of which microscopic examination reveals the presence of ovarian stroma. Dr. Davis records a case where the fœtus was found in a cyst developed from the ovary, and situated between the uterus and rectum.

In abdominal pregnancy the fecundated ovum develops in the abdominal cavity. An inflammatory new formation of very vascular connective tissue envelops the ovum in a sac, which, capable of great distension, may equal in size a gravid uterus at term. Very rarely this sac ruptures prematurely, and more frequently than otherwise the fœtus is fully developed. We have no satisfactory observation of a fœtus having lived more than ten months in the abdomen of the mother, though Dr. Bond states that a bright red ring encircles the umbilicus of the child when gestation has been protracted. These cases may terminate in a variety of ways. Should the fœtus live to the normal end of pregnancy, generally a kind of labour pain sets in, and often a decidua is expelled from the uterus. In the meantime the fœtus dies, and after death undergoes a variety of changes. Sometimes the body of the dead fœtus so irritates the walls of the sac that they inflame and suppurate, or even putrefy, and the consequent peritonitis kills the mother, or she perishes with hectic symptoms, exhausted by profuse suppuration. Should the peritonitis not become general the contents of the sac may perforate some neighbouring

hollow organ, most frequently the large intestine, sometimes the vagina or bladder, or even the stomach, as in a case recorded by Raneyer, may suffer in this way, or the disintegrative fœtus may escape through one or more fistulous openings in the abdominal wall. By whatever route the discharge takes place, the process of elimination is slow, and while it is going on, death may ensue from exhaustion or pyæmia.

When the walls of the sac are less irritated by the dead fœtus, a different series of changes take place—the soft parts undergo fatty degeneration, become partly reabsorbed, and the contents of the sac, after a lapse of some years, consists only of the bones of the fœtus and a quantity of calcareous plates, forming what has been termed a lithopæ-dion. Even in this state, however, the patient is by no means free from danger, and at any moment inflammation may arise, and be followed by suppuration and death. The name of secondary abdominal pregnancy has been given to those cases where the fœtus originally contained in the Fallopian tube, the ovary, or even the uterus, has escaped after rupture of its sac into the peritoneum and remained there. Dr. Barnes thinks that all abdominal pregnancies are primarily either tubal or ovarian, in which opinion he is opposed to that of most other observers. A case of abdominal pregnancy, unique at least in its mode of occurrence, has been recorded by Koeberlé. He had removed from a patient the body of the uterus, and part of the cervix leaving the ovaries. In the portion of the cervix that remained there was a fistulous aperture open-ing into the abdominal cavity; through this semen passed and produced an abdominal gestation. In some cases where the fœtus alone escapes into the abdominal cavity, leaving the placenta in its original place of insertion, a very remarkable preservation of the former sometimes occurs. Whilst inflammatory new formations cover the embryo on all sides, it may doubtless continue to live for a short time, and under these circum-stances the very vascular connective tissue closely approaches the skin on all sides, and the soft parts of the body are so marvellously preserved after death that after many years they show perfectly their normal structure.

The diagnosis of abdominal pregnancy is beset with difficulties. Along with the general signs of pregnancy, there is generally metror-rhagia; and this, together with repeated attacks of abdominal pain, due, probably, to intercurrent attacks of peritonitis, occurs more frequently than in tubal gestation. The abdominal enlargement is generally greater transversely, and the fœtus can be felt more distinctly than in normal pregnancy. Per vaginam, the cervix may be felt displaced by the pres-sure of the cyst, or fixed by perimetric adhesions. Either of these, Dr. Playfair thinks, is of great diagnostic value. We should certainly have recourse to the use of the uterine sound before undertaking any operative interference. In his paper on this subject Dr. Lawson Tait

says there are two points invariably to be noticed in extra-uterine gestation which had gone past the period—viz., a show during the false labour, and subsequent diminution in the size of the abdomen, due to the absorption of the liquor amnii. Before the death of the child extra-uterine fœtation may be mistaken for displacement of the normally pregnant uterus, during the early months, for pregnancy complicated with fibro-myoma or cystic disease of the uterus, and more rarely for pregnancy of one half of a double uterus. Indeed in this latter case, as Kussmaul has shown, a differential diagnosis may be quite impossible before death, and very difficult even after it. After the death of the child the diagnosis is more difficult, and the case might be confounded with pelvic hæmatocele and ovarian tumour, especially dermoid cysts, cancer, fibro-cystic disease of the uterus, hydatids of the uterus, and phantom pregnancy, with hernia.

Regarding the treatment of abdominal gestation, there is still some difference of opinion, though it is mostly admitted that no active measures should be adopted till the full term of development is reached, because there is not the same imminent risk of death from rupture as in the tubal form; and even if the destruction of the fœtus could be secured there would still be formidable dangers from subsequent attempts at elimination or internal hæmorrhage.

If menacing incidents occur when the gestation is so far advanced that a living child may be expected, or if the case has gone on to term and the child be alive, we are strongly advised by the best authorities to perform the Cæsarean section, as we may thereby save the child, and the risk to the mother is scarcely greater than in waiting. An incision large enough to extract the fœtus may be made in the linea alba, and, should there be no adhesions, the walls of the cyst should be stitched to the margins of the incisions to cut it off as much as possible from the peritoneal cavity. No attempt should be made to remove the placenta unless it be quite loose, as otherwise uncontrollable hæmorrhage, owing to the nature of its position, is likely to occur. The cyst should be carefully washed out with some antiseptic fluid. Schrœder advises us, when possible, to remove the whole sac, with the fœtus and placenta.

If the child has died towards the end of pregnancy the treatment is merely symptomatic. Any efforts of nature to eliminate the foreign body are to be assisted by incisions, and the removal of pieces of the fœtus from the abdominal walls, the vagina, or the rectum. The more extensive the adhesions of the sac to the abdominal walls, the more safely can incisions be made to remove its contents; and their formation may be favoured, as in Simon's method of treating hydatid cysts of the liver, by pushing in two trocars, and by dividing the bridge between them, thus causing a larger opening.

MR. F. T. PORTER.—Has Dr. Purefoy any statistics in his possession as to the number of cases in which Cæsarean section has succeeded in saving mother and child, in cases of extra-uterine fœtation? I also wish to know his opinion as to whether extra-uterine fœtation is more prevalent on the right or the left side. I have heard some state that it is more prevalent on the right side. I also wish to ask whether in the cases he has dissected, or has heard of being dissected, there has been any abnormal situation of the uterine end of the Fallopian tube—whether it was higher or lower than usual or occupying any abnormal situation with respect to the uterus?

The PRESIDENT.—I think Dr. Purefoy mentioned that some bones were discharged per vaginam. It might be interesting to know whether those bones passed through the body of the uterus or through the wall of the vagina, by process of absorption.

DR. DENHAM.—I am happy to say that our experience on this subject is limited, but that does not take away from the interest of the very able paper to which we have listened. I have had some slight experience of this unhappy form of conception. About a year and a half ago I brought under the notice of the Society a very distressing case of extra-uterine fœtation, which occurred at Rathmines. The patient was a young woman who had had either two or three children. She supposed herself to be four months gone in pregnancy, and was in excellent health, when one day, while she was undressing after a long walk, she suddenly felt weak and faint. A medical friend, who lived near, was sent for. The faintness passed away after the use of some stimulants. At five o'clock in the morning it returned, and I was sent for and met her medical friend in consultation. I found her in an extremely weak and exhausted state. She had one faint after another during several hours, evidently from some internal hæmorrhage, but from what source it proceeded we could not tell. There was no uterine hæmorrhage. The patient died, and it turned out that it was a case of extra-uterine fœtation in the Fallopian tube. Unfortunately, in a practical point of view, these are cases in which one's hands are tied. I believe we can do nothing. But while these cases are so distressing, they are all the more interesting on that account. When I was Master of the Hospital I had a case similar to what Dr. Purefoy has described, and removed all the bones of the head and several of the bones of the skeleton through the rectum. Evidently, the fœtus either died in the body of the uterus, or some adhesions took place between the extra-uterine fœtus and the rectum. The bones were very well formed, and their removal took place twelve or thirteen years after the period of conception. They were taken out with a forceps without much difficulty. During all the

previous years the poor woman had been in wretched health, and nobody knew what was wrong with her. She had constant discharges of pus, sometimes from the vagina, and sometimes from the rectum. After the removal of the bones, her health rapidly recovered. I had another case in ward 11 of the hospital, in which the woman, after perfectly natural labour, was delivered of a fine healthy child, but she had some tendency to hæmorrhage. The pupil in attendance, fearing that he had left some portion of the placenta behind, made an examination with his finger, and felt a very hard body in the cervix uteri. He directed my attention to it, and I passed up a forceps, and drew out a well-formed molar tooth with three prongs to it. My impression is that the woman must have had some unnatural conception, and that this was the last remnant of it. The woman recovered perfectly. In the case which I first mentioned, an eminent physician was sent for and could not come. He was sent for a second time, and arrived after the woman had expired; and he gave a most elaborate history of the case, describing it as one of perforation of the stomach or intestine, in which ulceration had caused some artery to give way. But, on a *post mortem* examination, I need scarcely say that his theory went for nothing.

DR. ATTHILL.—Dr. Purefoy's very carefully prepared paper has entered into every point of interest. Practically, there are but two points of interest in these cases—namely, first, as to the diagnosis, and next whether we are justified in interfering? In cases such as that detailed by Dr. Purefoy, when the extra-uterine fœtation only comes prominently into notice at a time when the child died, and the two first cases alluded to by Dr. Denham in which the bones came away, of course our treatment is simple enough—we endeavour to facilitate the exit of the foreign body. But it is quite different in cases in which we have reason to believe that extra-uterine fœtation has occurred, and that the fœtus is alive and in process of increase. The distinctions of tubal, ovarian, abdominal, and intestinal, are, for practical purposes, a great deal too refined. So far as treatment is concerned, we may consider these cases as either tubal or ovarian, or as being developed in the cavity of the abdomen. In the former class the result is nearly always fatal. In the other there is a fair chance of the mother surviving, and also a possibility of saving the child. Unfortunately, in the early stages, we have not the slightest certainty of being able to diagnose in what position the development has taken place. There probably is a greater amount of suffering and distress when the development takes place in the Fallopian tube or in the ovary, than when it has occurred in the abdominal cavity. But we can merely guess as to where the site of development is, and, therefore, are obliged to wait. The rule laid down by the best authorities, no doubt, is not to interfere. But there is a limit to that. And,

certainly, if the patient were suffering very great distress, and if I were satisfied that there was an ovum present, in certain cases, I think I should be justified in endeavouring to remove that which was nothing more or less than a foreign body. Dr. Purefoy has spoken of abdominal section. No doubt, that is justifiable, under certain circumstances, when we are satisfied that the child is alive and has reached nearly its full term. It is then right and proper to endeavour to save mother and child by abdominal section. That is the only course which then gives a fair chance of saving both mother and child. But if we proceed to operative interference in the early stages of pregnancy, the operation per vaginam is the proper one to attempt. Dr. Thomas's case is a most instructive one, both as to diagnosis and treatment. It was perfectly successful; but, judging from the details of the case, I think he would have been wiser to have been satisfied with removing the ovum and not attempting to remove the placenta. The result of attempting to do so was that profuse hæmorrhage followed, which nearly cost the woman her life. He had to inject perchloride of iron into the cyst, and the woman with difficulty recovered. One reason why I prefer the operation per vaginam·to making an opening through the abdominal wall is, that it is almost impossible to remove all that constitutes the foreign body—namely, the ovum, the placenta, the membranes, and so forth, including what we may call a false uterus, for the ovum is completely enfolded in a cyst which is to all intents and purposes a uterus. The result is that a portion is almost certain to be left behind, and that decomposition takes place. The evacuation of the products of decomposition through a section in the abdominal wall is very uncertain, whereas in the case of section per vaginam you have the likelihood of the discharges being safely evacuated through the vagina.

DR. M'CLINTOCK.—I have had no experience of extra-uterine fœtation, and perhaps may congratulate myself on not having encountered a case of the kind. Dr. Purefoy has stated, I think on the authority of Dr. Barnes, that many cases of what has not been very correctly called retro-uterine hæmatocele, but which would be better designated as pelvic hæmatocele, have had their origin in extra-uterine fœtation, and an early rupture of the cyst, causing extravasation of blood into the peri-uterine cellular tissue, or the peritoneum. I have seen a few cases of pelvic hæmatocele, and in two of them the history of the case up to the formation of the tumour and the development of the hæmorrhage was clearly that of early pregnancy. Each of the patients had had two or three children previously and was living at the same time with her husband; and in each case also the woman had passed two or three periods without menstruating, and had come to the conclusion in her own mind that she had conceived. They were suddenly attacked with pain, prostration, and sickness, and a tumour rapidly formed either behind or beside the uterus.

I am satisfied that at least two of the cases of pelvic hæmatocele that I have seen were really, in the first instance, examples of some form of extra-uterine fœtation ; and that at an early period, perhaps a month or six weeks after impregnation, the cyst burst and caused the extravasation of blood. The blood in both cases was discharged by the rectum in large quantities, and the patients recovered. This, I think, is a very interesting pathological point in connexion with extra-uterine fœtation, and bears on the development of another disease of a very important and interesting kind—namely, pelvic hæmatocele.

DR. PUREFOY.—With respect to Dr. Porter's question as to successful abdominal sections, I cannot say that I have any statistics on the subject. In the first place the conditions have been seldom favourable to the immediate performance of so very serious an operation, and up to the present no one has had the courage to perform it. At the same time it has the sanction of all our good authorities. I am not aware that the occurrence of extra-uterine fœtation has been noticed more frequently on the one side than on the other. As to Dr. Porter's question whether, in the case I have submitted, the fœtal bones passed through the uterine wall or not, my belief is that they passed through the vagina. There is a cicatrix in the vagina, to the left of the cervix, through which I believe they passed. There are a few cases in which extra-uterine fœtation is believed from the history of the cases to have occurred in connexion with pelvic hæmatocele, there being a possibility that, in the early months, one may be mistaken for the other. In the case—mentioned in my paper—of a girl who was under my care some months ago, a physical examination of the case suggested pelvic hæmatocele. The hæmorrhage came on after unusual exertion, and at the time I looked on it as a case of hæmatocele ; but, at the same time, I was struck by the circumstances that the breasts were considerably developed and that the uterus was very much more enlarged than it usually is in cases of hæmatocele, and the conditions of the case did not preclude the possibility of pregnancy.

Specimen of Extra-uterine Polypus. By J. DENHAM, M.D., Ed., L.K.Q.C.P.

THIS specimen is an interesting example of polypus. Some days ago I was called in by Dr. O'Flaherty to see the patient from whom this tumour was removed. At first the lady, with that innate delicacy which characterises our nation, would not permit a vaginal examination, and Dr. O'Flaherty was obliged to treat her for ordinary uterine hæmorrhage. However, after she became very much worn down by repeated hæmorrhages, she permitted a vaginal examination, and then he discovered the existence of a very large tumour. The following day I saw her, with Dr. O'Flaherty ; and, as she had been greatly exhausted by the previous hæmorrhages, we determined that we would wait a few days before

attempting its removal. The day following, the hæmorrhage returned unexpectedly, and with unusual severity. As we saw that waiting any further might endanger the patient's life, we proceeded on the following morning to remove the tumour. We found that it was firmly impacted on the pelvis, and that it evidently had escaped through the os and come down into the vagina. Having placed the woman under chloroform, with the assistance of Dr. O'Flaherty, I succeeded in passing an ecraseur over the body of the tumour. We cut through the pedicle, which was not very thick, as you observe from the appearance of the tumour. We found the tumour still as firmly fixed as it was before, and that no power that we could apply with safety would draw the tumour down, or even move it on its axis. We came to the conclusion that we had not succeeded in cutting through the neck. We passed the wire twice again over the tumour with an equally unsuccessful result. However, I presume the manipulation we exercised in trying to get the wire round caused the parts to dilate a little and yield. She was an elderly, unmarried lady, and the parts were extremely rigid, and it appeared like a case of tedious labour with locked head. However, ultimately the tumour became movable. The difficulty then was to remove it. We had a pair of Dr. Madden's small forceps, with which we grasped the tumour, but even with that instrument and the vulcellum we failed to draw it down. Ultimately, however, we got it down. It was then very much larger than it appears now. We were obliged to slit the perinæum with a pair of scissors, in order to get it away. I never used greater force with a forceps than I had to do in this case, so firmly locked was the tumour. Dr. O'Flaherty will be able to state how the patient is going on.

Dr. O'Flaherty.—After the removal of the tumour by my friend, Dr. Denham, the case assumed a favourable condition; but from the exhaustion of the patient, caused by previous hæmorrhages during the last two or three years, the result was at first very doubtful. I think, however, that now the patient is in a fair way of recovery.

Dr. Atthill.—The case seems to illustrate one point in a very interesting manner. In the great majority of cases of intra-uterine polypi, as soon as the tumour is extruded from the uterus, the hæmorrhage ceases to a great extent, but it appears in this case it was not so. Within the last ten days I removed a polypus nearly as large as the one before you from a lady whose case was somewhat analogous to the present—with this difference, however, that her sole symptoms were profuse and exhausting leucorrhœa; she had no hæmorrhage whatever. An attempt at spontaneous cure was being set up, for the polypus was beginning to slough; and the probability is that had it been left alone it would have sloughed away and run her down so as to cause death.

Erysipelatous Metritis. By THOMAS D. FINUCANE, L.F.P.S., Glasg.

MR. PRESIDENT AND GENTLEMEN,—As you are aware, the subject of my paper this evening is "Erysipelatous Metritis," the importance of which has been lately brought rather forcibly to my own mind by the occurrence of two deaths of young women after their confinements, as also a third death in the same neighbourhood—the virus in this case having been brought from a scarlatina patient. The nurse who attended on the two first cases (I may mention she was not a qualified one) had a daughter ill in erysipelas at the time of her attendance. She did not conceal this, but told the first family of it, "thinking," as she said, "that it was no harm." It has been the subject of much inquiry with myself, why the case of erysipelas as also of scarlatina should recover, and the poison or virus conveyed or transmitted should prove fatal in every case of confinement with which it has been brought in contact, so far as I know. The patients were not in a position to have a medical attendant, unless in an emergency; and in such I was sent for to see one of those cases, the symptoms and history of which I now submit. I first visited this patient on the evening of the 3rd of November, 1876. She was then in the first stage of labour, progressing favourably—so that she was confined on the morning of the 4th. The following day (the 5th) she had severe abdominal pains, which were treated by the nurse—the usual terebinthinate treatment followed by a full opiate, which gave relief. On the 6th, however, I was again sent for, and found the patient in a critical state—very rapid pulse; restlessness; suppression of the lochia; breasts almost empty; much tympany; no vomiting. Repeated the terebinthinate enema, linseed poultices, full opiates, with minute doses of submuriate, chicken-broth. On the morning of the 7th, a diffused rash showed itself over the thighs and hips of the patient. She grew rapidly worse; felt as if she must burst; could not lie in any position for any length of time; the tension of the abdominal walls was quite remarkable and unyielding. The pulse on the evening of the 7th was peculiar—at times it seemed almost to disappear and then to become more distinct. She died on this, the fourth night after her confinement, the functions of kidney and bowels being quite as they ought to be during her illness. In a few days after her death, her sister, who was in constant attendance upon her, was covered with a rash, and has been suffering, almost ever since, with articular and muscular pains—said by her medical attendant to be a regular rheumatic attack.

[The author quoted at length the opinions of Drs. Austin Flint, Bennett, and Bryant, on epidemic erysipelatous fever and erysipelas, and of Dr. Carpenter on the operation of medicinal or poisonous substances, and their determination to special parts and organs of the body. He observed that many facts connected with the action of poisons on the

economy tended to confirm the Hallerian doctrine of irritability. Whether it be the inhalation of a poisonous gas, or the contact of an irritant, the inherent irritability of muscle is roused to rebellious action. In accordance with this theory, the suppression of the lochia would depend on muscular action. In conclusion, he remarked that the position of his patient in a dark confined room was much against healthy respiratory interchange, so essential for our well-being at all times; and, therefore, he suggested the propriety of the admission of more light into our dwellings and hospitals, having regard to its purifying influences, and to its wonderful action upon some poisonous substances.]

Dr. Atthill—At this late hour it is hardly possible fully to discuss Dr. Finucane's paper. It strikes me that the name "erysipelatous metritis" is not perhaps one borne out by the pathology of the case. I think this was one of those cases included in the general term "puerperal fever," which I look on as being due to autogenic septicæmia, which is, in my opinion, a very common cause of death after childbirth. This woman does not appear to have been exposed to any infection, yet she was attacked a few hours after delivery, showed symptoms of blood-poisoning, and sank rapidly. I think the only way to account for these cases is to suppose that the patient was self-poisoned. Dr. Finucane has alluded to scarlatina; I do not think it is so much to be dreaded by puerperal women as is generally supposed. I never yet knew a case in which the infection of scarlatina was carried to a puerperal patient, although I do not deny that it occurs occasionally. Within the last twelve months a good many deaths from metria occurred in the Rotunda Hospital, chiefly in March last; and we had at the same time also several cases of scarlatina, one or two of which were sent to fever hospitals; one at least was kept in the wards, but in no instance did a patient in the hospital contract the disease. The patient we kept in the hospital had the disease very mildly; she made a most rapid recovery, being as well on the eighth day as women in the same ward with her who had nothing the matter with them save the effects of their confinement.

Dr. Doyle.—In 1874 I attended a woman the day she was delivered; and the next day I found that she had in the bed with her one of her children, aged six years, who had well-marked symptoms of scarlatina. I wanted the mother to send the child out of the house, but she would not. That child died; and in about a fortnight or three weeks afterwards another of the woman's children, who had got the disease, died of secondary affections and dropsy, yet the mother never got the scarlatina, and recovered perfectly.

Dr. More Madden.—I think it would be most dangerous to let the idea go forth that scarlatina is not a very dangerous disease to parturient

women. If that idea were generally acted on, medical men would be very apt to attend women in labour while they might at the same time be in attendance on cases of scarlatina. Dr. Atthill, whose experience has been very much wider than mine, has stated that he never saw a case of scarlatina producing puerperal fever. I regret to say that I have known a case of a woman dying from puerperal fever contracted from one of her children who was lying in the same room with her suffering from scarlatina. At least if it was not *propter hoc*, it was a case of *post hoc*. It was the case of a lady living in Lower Gardiner-street, whom I attended about three years ago. Scarlatina was epidemic at the time, and she would not let her child, who was ill of it, out of her room. On the third day she shivered and got all the symptoms of puerperal fever, of which she died. I believe that the disease was communicated to her by the child who slept in the room with her. Therefore we should be very cautious before we depart from the idea that scarlatina may be communicated under the form of puerperal fever to a woman.

DR. ATTHILL.—I do not deny it; I only say it is not so much to be dreaded. I should be very sorry to attend patients in scarlatina. I do not do it. At the same time I do not think scarlatina is so frequent a source of puerperal fever as is generally supposed.

DR. M'CLINTOCK.—It may be that a pregnant woman is not very susceptible of scarlatina. That is one question. Another is, as to the danger of scarlatina attacking a woman in childbed. It does not follow that because a parturient woman is exposed to the danger of scarlatina she will take the disease. She may not do so if she has had scarlatina previously; I believe that the poison of scarlatina strongly predisposes to the production of puerperal fever if it is not actually capable of producing it. As to the danger arising from scarlatina when it attacks a parturient woman, I make this statement, founded on very large and painful experience, that when scarlatina is developed in a puerperal woman before the third day of childbed, it is a more fatal disease than puerperal fever itself; and I would much rather encounter the latter than the former. If the scarlatina does not develop itself until after the third day, there is a tolerable chance of the woman's recovery. While I say this, I would make an observation *apropos* of what Dr. Atthill has stated. I think that at the present day the idea of the communicability or transmissibility of contagious diseases, or of their being carried about by medical men, is enormously exaggerated. The extent to which this idea is now taken up by the public is absurd. If a man is now known to have had a case of puerperal fever, even though it might have been three months before, and down in a remote part of the country, he is in danger of being tabooed and excluded from practice. Some months ago

I saw a case of puerperal fever, and she died. Two months afterwards the husband of one of my patients called on me and asked me if I had seen that case; I replied that I had, twice. He said he had been told by some of his wife's friends that my attending her in her confinement, which was approaching, would be accompanied with risk. I said— "Does your wife know of this? for if she has the slightest fear on her mind you had better get somebody else." He said that she knew nothing at all about it. I told him there was as much chance of my giving puerperal fever to her as to the man in the moon; that no well-educated medical man would endorse the idea that it was in the remotest degree possible that I could convey it; and that I had attended ten or twelve patients since, every one of whom had perfectly recovered. If the communicability of contagious diseases exist to so great a degree as is now popularly supposed, no person is safe—you should never go in a railway carriage, or to a public meeting, or to a place of worship.

The PRESIDENT.—I think we ought to be very much obliged to Dr. M'Clintock for what he has said. The doctrine now prevails that certain classes of diseases are communicated by poison, and that you cannot have them without it. If all these diseases depend on poison it is only a change of name; but the gentlemen who advocate the doctrine are bound to demonstrate that the poison is a reality, and not to depend on a mere hypothesis. I believe the profession are doing mischief to the public and to themselves by this way of speaking. I have suffered in that way myself. I believe that the poison doctrine cannot be maintained, and is a mere hypothesis.

DR. DENHAM.—I think we have a great deal to learn on the subject of erysipelas, scarlatina, and puerperal fever. My own opinion is that the poison is very much of the same character, and that the same influence that would produce erysipelatous inflammation in one patient would produce scarlatina in another, and puerperal fever in a third. I have not the slightest doubt that in the great majority of cases the poison has been communicated to the patient before the labour set in at all. I must say that I differ from Dr. Atthill, for I look upon scarlatina in the puerperal as a most dangerous form of disease. Out of fifteen cases of scarlatina that occurred during my mastership twelve died.

DR. HENRY KENNEDY.—Several years ago I had a very large opportunity of examining a good many of those cases of puerperal fever in connexion with the subject which Dr. Finucane has so elaborately brought before us; and the appearance of the bodies frequently showed an erysipelatous state of the surface. I repeatedly saw it coming out on the vulvæ, the nates, and the groins. I do not believe such a disease could have spread from erysipelas. Some of the worst cases I ever saw were

complications of scarlatina with puerperal fever, these being two distinct diseases. The chief remedy we employed consisted of very large doses of barm. The cases were of a very extreme character. I rather agree with what has fallen from Dr. Denham as to the great fatality of scarlatina when it attacks a puerperal patient; but it should be remembered that scarlatina and puerperal fever are distinct diseases, and should not be jumbled up together. There is no connexion between the two at all. Scarlatina may attack a puerperal patient, but we have no evidence to support the proposition that scarlatina poison will engender puerperal fever.

Dr. Finucane, in reply, said :—My quotations have been from men of eminence. It was not so much to instruct others as to be instructed myself that I brought my paper before this learned Society. The difficulty I experienced was that the persons who communicated the scarlatina or the erysipelas to women in their confinement recovered, whereas the parturient patients were sure to die. What, then, has become of the power ordinarily possessed by the system of eliminating the poison? Why should the transmitted influence—whatever it be—prove fatal in the case of the parturient patient, if it have not the power of interfering with the eliminating power of the body. My object has been to show that the poison affects the tissues and not the blood.

The Society then adjourned.

IODISED PHENOL IN UTERINE DISEASES.

Dr. Battey, feeling the need of a combination which should possess not only the properties of a local escharotic, but those of a local and at the same time general alterative also, devised, several years ago, the following formula :—Take of iodine one half ounce, crystallised carbolic acid one ounce—mix and combine the two by gentle heat. This has proved a very satisfactory application in uterine cancer. Lint or cotton is saturated with the remedy and applied to the cancerous surfaces, the sound parts being protected by a cotton tampon. The application may be repeated in four to seven days. When it is desired to get rid of much fungous growth, the dead tissue is removed by the curette, and another application made without waiting for the separation of the slough. A weaker preparation has been very fully tested by the writer in a large number of cases, and in a variety of uterine disorders—*e.g.*, chronic affections of the cervix, the cervical canal, and the endometrium; uterine hypertrophy, and subinvolution. This solution is prepared by mixing iodised phenol, one and a half ounce, with crystallised carbolic acid, one ounce, and water two drachms. It may be used both in its full strength and in various degrees of dilution with glycerine.—*American Practitioner*, Feb. 1877.

TRANSACTIONS OF THE ULSTER MEDICAL SOCIETY.

SESSION 1876-77.

President—RICHARD ROSS, M.D.
Honorary Secretary—WILLIAM WHITLA.
Tuesday, January 9, 1877.
DR. ROSS, President, in the Chair.

Malignant Tumour of the Orbits. By DR. FAGAN.

DANIEL MACKLIN, a young anæmic child, aged seven years, was admitted under my care at the Children's Hospital on the 13th October. He was suffering from diarrhœa at the time, and was delicate for four or five months previously. There was a tumour on the left temporal region, limited by the boundaries of that fossa. The eye was protruded and ecchymosed, and he sometimes complained of it as being painful and itchy. The only history I could get in connexion with the case was that, about six weeks previous to my seeing him, the eye was noticed to be somewhat more prominent than its fellow, and the temporal region somewhat fuller than the opposite one. The only cause the parents could assign for this condition was that, a short time previously, while hammering something with a poker, it rebounded, giving him a sharp blow on the temple. It caused acute pain at the time, but did not afterwards trouble him. Careful manipulation of the swelling conveyed an indistinct sense of fluctuation, as if matter existed deeply in the tissues. This was my diagnosis at the time, and it was confirmed by some other medical men who saw the case with me. That it was a case of deep-seated chronic abscess I considered most probable for the following reasons:—First, the history of the case would point to the blow on the temple as being the exciting cause; next, the symptoms following might be those of inflammation, of a sub-acute form, of the deep cellular tissue, the matter formed being bound down by the dense structures in the temporal region, made its way through the parts that offered least resistance. This I believed to be through the spheno-maxillary fissure into the orbits. This condition of things, I satisfied myself, could account for the protrusion of the eye-ball and the swelling in the temporal region. Accordingly, after a few days, when I got the lad's health into a better state, I put him under the influence of ether, and made an incision over the great wing of the sphenoid, right down to the bone. Nothing came away but a little blood. I then passed a narrow-bladed knife along the outer wall of the orbit, back into the spheno-maxillary fissure, and with the same result. Water-dressing was applied, and the lad put back to bed. No bad effects followed the wounds

made in exploring the parts. They healed up in a few days. When I satisfied myself that the swelling was not due to an abscess, I was inclined to look on it as malignant. The only treatment I now employed was to protect the parts, keep them clean, and support the child's constitution. The temporal enlargement went on steadily increasing. The eye-ball became still more protruded, the conjunctiva very vascular and swollen. The cornea, at first clouded, became ulcerated, and covered with crusts of dried secretion; the tears flowed over the cheek, and were of a dirty, muddy character. He slept pretty well, and eat well, and complained only occasionally of pain. This was his condition on the 29th, about a fortnight after his admission, and at this time I noticed a swelling in the right temporal region, of the same character as the other, in its earliest stage. In two or three days this increased, and the right eye-ball began to protrude. I had now no doubt about the malignancy of the affection, and acquainted the parents of the hopelessness of the case. The child was removed home, and I did not see him until November 20th, when I got photographs taken. He then presented the appearance that is pretty well shown in the likeness— that is, the former condition of parts very much aggravated. He appeared completely broken down in constitution, and exhibited in his appearance what is supposed to be the characteristic cancerous cachexia. His appetite was bad; he became peevish and irritable; his sleep was restless and irregular, and he sometimes awoke with a scream. Three days before his death he got a short, teasing cough, the lower extremities became œdematous, and he complained of difficulty of breathing and pain about the chest. He died on the 9th December. This case, Mr. President, I have found to be most instructive. It shows that no matter how careful the surgeon may be in considering the causes and symptoms, he may be mistaken in his diagnosis; and, what is of more importance still, he may hold out hopes, or predict gloomy results, only to find himself and his patient disappointed in the end, or, taking hasty action on his decision, he may cause an unnecessary sacrifice of parts. In this particular case the action taken on the diagnosis could not in any way leave it in a worse condition, while it assisted very much in throwing more light on the subject. The importance of forming a correct diagnosis is more evident when one of the limbs is the subject of a similar tumour, and that the question of amputation has to be taken into consideration in its treatment. Mr. Holmes, in speaking of the difficulty of establishing a diagnosis between ostitis and malignant disease, says that he has known at least three cases in which amputation at the hip-joint had been contemplated for a malignant affection of the femur, which turned out afterwards to be of a simple inflammatory nature. And after going over the various symptoms that aid one in their diagnosis in such cases, he says one practical consideration must occur to the mind of anyone charged with

the responsibility of a case of this kind—viz., that no operation ought to be performed while any doubt whatever rests on the diagnosis. If the case is one really of cancer, the operation is, at the best, of doubtful value; if it is ostitis, it be worse than superfluous.

Dr. J. W. Browne said he had the advantage of seeing the patient, and when he saw him he thought it was a case of abscess at the back of the orbit, pushing forward the eye-ball, and that it was the result of the injury, which was so clearly stated by the parents. He said there were cases narrated by Mackenzie and others where soft cancer (encephaloid disease) had immediately followed injury. He considered this case one of these, and pointed out the resemblance between this and strumous disease, the differential diagnosis between them being that in cases of strumous disease, where the eye-ball bursts, it collapses, while in the encephaloid it increases in size. The disease had probably commenced in the brain or ethmoid bone.

Dr. Dempsey thought that if the disease had commenced in the brain cerebral symptoms would have shown themselves.

Dr. Wales saw two cases of cancer in young children of cerebral origin. In both death resulted from secondary deposits—in one in the thoracic, and in the other in the abdominal viscera, and in both cases no cerebral symptoms showed themselves, the children remaining clear till the end.

Dr. Coates thought if this was a case of cancer, it was certainly most unusual in one so young.

Dr. Whitla said Dr. Fagan had referred to the use of the ordinary hypodermic syringe which he (Dr. Whitla) had tried in this case to clear up the diagnosis. He had used it often, and never saw it fail to detect pus in any case where the after-course showed there had been pus. He saw it recommended first for the diagnosis of fluid in the pleura. He used the syringe in this way—after seeing the piston was air-tight, he half filled the cylinder with water, introduced the needle into the suspected abscess, and injected three or four drops of water to clear the needle, then a few turns of the screw brought the pus ascending through the clear water in the cylinder. He thought that, in ordinary practice, it was vastly superior to the aspirator. The needles which he used he had made by Gardner of Edinburgh. They were of extreme fineness, and caused little pain.

Compound Comminuted Fracture of Knee-joint.

Dr. Murney exhibited a specimen taken from a patient who had sustained a compound comminuted fracture through the knee-joint three months ago, by the falling of the framework of a ram. The injury was followed by intense collapse, in which condition he was admitted into hospital, and it was the unanimous opinion of the surgeons that it would

be unwise to amputate, the patient was so low. In this condition he remained for some days ; and when the question of secondary amputation came to be considered, he was so prostrated by the profuse discharge which poured from the wound, as well as by a most obstinate and wasting diarrhœa, night-sweats, and loss of appetite, that the question of opera-tion could not be entertained till about three weeks ago, when he amputated by Carden's method, and the patient made an excellent reco-very, being now nearly convalescent. The specimen showed that the patella was removed, and the section made through the lower-third of the femur at its junction with the middle one-third. The fracture was T-shaped. The inner condyle of the femur was detached, the outer was externally rotated, and partially united to the shaft. The knee-joint was found partially ankylosed, being united by a kind of fibrous or ligamentous union, and which never could have been of any service to him. Three or four pieces of bone exfoliated, and became sources of great irritation.

DR. FAGAN asked if the fracture had been diagnosed at first as passing through the joint; if so, after the shock had passed off, he thought primary amputation should certainly have been performed. The fact of his surviving, and the after-history of the case, proved this. With Dr. Murney's remarks about tying large veins he quite agreed, but preferred torsion. He had twisted the femoral four times, and was pleased with the result.

DR. COATES (House Surgeon) said the idea of primary amputation could not be for one moment entertained. The shock lasted for five days, during which he was fed upon stimulants.

DR. WALES thought the case interesting from a medical point of view, and inquired was the liver enlarged or the urine albuminous ?

DR. BROWNE, who had seen the patient a few days after the injury, when he first came under his care, found the limb up in the long splint, which he at once changed for Macintyre's. The liver was enlarged. Urine contained no albumen; discharge was profuse, and symptoms of pyæmia soon showed themselves. He was told amputation was out of the question from the first. The diarrhœa was most obstinate. He found lead and opium had some effect; but this soon produced drop-wrist, and marked the gums, and had to be discontinued. He was most anxious to amputate, but never saw a suitable opportunity. The patient then passed into the hands of Dr. Murney, and it was not for some time after this that any hope of a successful operation was entertained.

DR. ROSS thought the case had a very successful termination. He always looked upon the knife as an opprobrium ; but, certainly, it seemed to him, in this case, that it had saved a life. He questioned very much the issue if the knife had been used at first, from the description which he had heard of the patient's condition.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

President—THOMAS HAYDEN, F.K.Q.C.P.

Secretary—E. H. BENNETT, M.D.

Aortic Aneurism ; Hæmorrhage into Pericardium.—SURGEON-MAJOR JACKSON, C.B., Army Medical Department, exhibited a specimen of aortic aneurism, fatal by hæmorrhage into the pericardium, and gave the following details :—J. A., shoeing smith, Royal Horse Artillery, age twenty-nine, service, nine years, came sick to the station hospital, Phœnix Park, on 13th May, 1876, complaining of some difficulty of breathing, much increased by slight exertion. His face was naturally pale; body well nourished. His medical history-sheet noted his habits regular and temperate, and conduct good. His admissions to hospital during his service were for trifling ailments, with the exception of one attack of syphilis, from which he suffered in 1870, but as the treatment was merely local, and there was no record of a secondary affection, it is probable the disease was non-infecting. He had no admission for rheumatism. On examination his pulse was found to be 140, soft and weak ; the temperature in the forenoon 99°; area of cardiac dulness much increased, and a bruit over apex of heart with first sound was detected. In the evening of same day his pulse still reached 140, temperature 101·4°. On next day the pulse still continued 140, and bruit over apex more distinct. The case was, after this, under the charge of Surgeon-Major Ward, and on May 26th I find the following remarks in the case-book :— "The palpitation of the heart continued, much increased by exercise. He was a great smoker. Area of precordial dulness much increased, and a friction murmur was heard at times after the first sound. No difference was found in pulse at either wrist, nor was there marked pulsation in carotids." The bruit soon disappeared, but as the pulse still retained the quick action and weak tone he was brought before a medical board on the 15th of June. As no organic lesion was obvious except the increased area of dulness, instead of being discharged the service, he was recommended a sick furlough for two months. He had little treatment on this occasion. In the first instance bromide of potassium was given. We then lost sight of him until the 30th of November, when he was brought to the hospital about 10 a.m. He was at once carried from the ambulance and placed in bed. The orderly noticed that he had a slight convulsive attack, and ran for a medical officer; and, although we were in the wards at the time, before we arrived at his bed-side he was dead.

He lay on his back; face extremely pale; pupils much dilated. I may mention that Surgeon Frazer and I hazarded an opinion that the cause of death was probably hæmorrhage into the pericardium. From the notes which were brought with J. A. to the hospital, and a letter since received from the medical officer in charge of the Artillery, it appears that the fatal attack commenced on the 29th October. He complained of not feeling well on that day, and was excused working in the smith's shop by the farrier. When seen by the medical officer at 10 p.m., J. A. was cold, clammy, and restless; no pulse or heart's action to be detected. He told the hospital sergeant that he felt it was all over with him, as during the day, while lying on his bed in the barrack-room, he felt "something had gone like the crack of a whip in him." After this he sprang up and fell on the floor. During the night he had constant vomiting. The treatment was stimulants, mustard externally, and hot-water bottles to feet.

Post mortem examination, twenty-six hours after death.—On opening thorax the lungs were found natural, but extensive pleuritic adhesions, which were broken down with some difficulty, surrounded the left lung. The pericardium occupied a much larger area than normal. Near the apex of heart extensive patches of a dark colour showed where blood had been poured into the cavity. The pericardium and its contents were altogether nearly as large as a bullock's heart. The viscera being removed from thorax, Surgeon Frazer proceeded to separate the pericardium, which was found to be adherent to surface of heart, except where the extravasation of blood had taken place; this was to about the amount of twelve ounces, chiefly firm, dark coloured, with some portions of decolourised fibrine. The blood extended from the apex of heart, in a diagonal direction, to the base, where the rupture had occurred posteriorly. As we were anxious to have the specimen submitted to an authority who had a special knowledge of affections of the heart, I applied to the President of this Society, Dr. Hayden, who was so good as to examine the heart; and the following notes were taken down during the dissection from Dr. Hayden's remarks:—"On slitting up aorta small patches of atheroma were found; on making a transverse section of left ventricle the mitral valves were found natural, the walls of ventricle itself showing merely concentric hypertrophy. Of the aortic valves, two were normal, while one, the anterior and right valve, was thickened at edge and lax. The aneurism started from sinus above this valve, the opening into it being about the size of a florin; the edges of opening perfectly smooth and even. The sac was partially filled with fibrine of recent formation, not laminated, but hanging loose. The walls of left ventricle were rather thin, and decolourised fibrine descended in a ribbon from right auricle. The sac of aneurism descended in front of right auricle, then stretched to right side, completely surrounding the

right auricle, and also enveloping the last inch of descending cava, which it had dissected out; it not only surrounded the right auricle in front and right side, but also in part posteriorly. The coats of right ventricle were thickened by a layer of fat several lines in breadth, and near the apex the walls were chiefly composed of fat only. On trying the aortic valves with water they were found not capable." Liver and kidneys natural.—*December* 9, 1876.

Pyæmia.—DR. WARREN said: I beg to exhibit a specimen which, although not taken from the human species, yet is, I think, of sufficient interest to warrant me in bringing it under the notice of the Society. These viscera, Sir, were removed from the body of a pet monkey, the property of my colleague, Dr. Swan, to whose kindness I am indebted for the specimen. The case is one of embolic pyæmia.

The *history* is briefly as follows:—About a fortnight before the animal's death he was observed to be constantly picking at his right side, in the iliac region; this was first attributed to natural causes, but on examination a small opening was observed in the skin, through which a purulent discharge exuded. This was attributed to the irritation of the strap by which he was fastened, and it was accordingly removed to his neck, but he continued to tear and irritate the part, and its condition became so serious as to require free evacuation by incision. He now obtained temporary abatement of the symptoms, but in a few days he became in much the same state as before, and several times during the day would become insensible, and fall from his perch with violent clonic convulsions, which lasted about two minutes; he would then recover consciousness. It was now deemed necessary to again evacuate the pus by free incision, and again all the urgent symptoms disappeared for two days, but only to return with increased violence. It was now observed that he at times became totally blind, and would grope about for his food; this would disappear, and he recovered vision in about fifteen minutes. The convulsions now became more frequent, alternating with loss of vision; and finally the animal gradually sank, and expired last Thursday without any convulsions.

Treatment consisted in the evacuation of the pus by incision upon two different occasions, and supporting the animal's strength by a liberal allowance of stimulants, of which he was extremely fond, especially sweet punch.

Autopsy, twenty-four hours after death.—The body of the animal was submitted to my colleague, Dr. Bookey, whom I assisted in making the *autopsy*. On the lateral dorsal aspect of the iliac region was observed an irregular and ragged opening, through which thick creamy pus exuded. Upon opening the abdominal cavity, a large sacculated abscess, about the size of an orange, was discovered; this had pushed

the peritoneum before it, and passed backwards as far as the ver-
tebræ, upon which it impinged; this abscess communicated freely with
the external aperture. The liver was of the normal size, but deeply
congested and friable. In the right lobe an enormous abscess existed,
which protruded from the outer surface of the lobe, and had contracted
very dense and firm adhesions to the inner surface of the ribs. A second
abscess was found upon the outer surface of the left lobe about the size of
a filbert. Both kidneys exhibited numerous small abscesses; spleen normal.
Upon opening the thorax we found the lobes of the right lung studded with
numerous "hæmorrhagic infarctions," especially the lower lobes. The
left lung was perfectly healthy. An abscess was also found in the right
lateral lobe of the thyroid gland. Upon examining the brain we dis-
covered a large circumscribed abscess in the under-surface of the right
posterior lobe of the cerebrum, otherwise the organ was healthy; and,
lastly, we found another abscess in the soft tissues at the root of the penis.
There was no icterus whatever. There was no effusion of pus in any of
the joints.

I think, Sir, the points of interest in the above may be summed up
thus :—Firstly, the total absence of jaundice so frequently observed in
pyæmia, especially when abscesses exist in the liver; secondly, that with
the exception of the kidneys, the abscesses were unilateral, being entirely
confined to organs of the right side of the body; thirdly, the non-
implication of any of the joints; fourthly, abscess in brain, rare. The
convulsions and loss of vision probably depended upon the formation of
abscess in the brain.

The above case was evidently one of embolic pyæmia, or "embol-
hæmia" of Weber, going on to "pyohæmia multiplex" of Heuter, or the
formation of metastatic abscesses in remote organs. I think the starting-
point of the disease may fairly be attributed to the irritation of the girth
round the loins of the animal causing an inflammation of the soft tissues,
terminating in abscess, which, burrowing downwards, pushed the peri-
toneum before it, and finally caused septicæmia by absorption. The
absence of jaundice may in all probability be attributed to the variety of
pyæmia which was met with in the case, being one of embolic pyæmia,
as distinguished from the variety in which the hæmatin of the blood
becomes disintegrated, in which we so generally find icterus without
metastatic abscesses; but in embolic pyæmia we find, as in above case,
metastatic abscess without jaundice. The hæmorrhagic infarctions were
caused by embolic plugging of pulmonary capillaries, and would have
finally caused abscess. Owing to the period of time which had unavoid-
ably elapsed between the death of animal and the necropsy, I considered
it useless to examine the thick viscid cream-like pus, as it would be
almost impossible then to ascertain the variety of bacterium which
determined the nature of the pyæmia.—*December* 9, 1876.

Excision of the Knee-joint.—MR. TYRRELL said: Last autumn a boy, aged sixteen years and six months, was sent from Mayo to be under my care, for the purpose of having excision of the knee-joint performed, if possible. He was stout, healthy, and very tall for his age. On his admission the affected limb presented the appearances which are very well seen in the cast. The muscles of the leg were somewhat attenuated. There was ankylosis with a very remarkable projection of the condyles of the femur, and an inconsiderable displacement of the tibia backwards. He stated that the disease had lasted about seven years; that it alternately grew better and worse; and that, about a year ago, in consequence of a sudden wrench, it became extremely bad, and grew into the state which the cast shows. I explained to him and his friends the danger of the operation, and that it was not absolutely required in his case, as he suffered no pain; but he said he was obliged to carry a useless appendage, which prevented him from following any trade, and that he would prefer to have it lopped off. I therefore determined to excise the joint, and did so on the 4th of October. I removed the lower end of the femur without disturbing the patella, which was firmly ankylosed at the time: the former presented the remarkable appearance described by Volkmann in some cases of disease of the knee-joint, in which its inferior articular extremity grows disproportionably, perhaps from the pressure of the head of the tibia being taken off it. In such a case as this you could not effect a reduction and replace by force the bones in position from the altered characters of the articulating extremities. I then removed the portion of the tibia you see here. On endeavouring to bring the ends of the bones together, I found that I could not do so. I then removed another slice of the femur with an equally unsatisfactory result. On removing a third thin slice of the femur, and pressing the bones together, I found that I could not only bring them into contact but that they pressed against each other. That would not do, as it would have produced necrosis from undue pressure. Rather than take away any more of the femur, I determined to divide the hamstring muscles. I did so, and also divided very freely a large mass of the thickened fascia—the bones fell into their places. Another accident, as I may call it, occurred, which was very instructive. Before I made the last section of the femur, the assistant, who held the thigh, drew the soft parts with such force as to cause the periosteum to recede from the condyles to the extent of half an inch all round. There are surgeons who would have considered it necessary to remove that portion of the condyles uncovered by periosteum, as it is generally thought that if a bone is not covered with periosteum it must die. That, I know, is not the fact. It has sometimes occurred to me, in amputating through the femur, that the bone has been stripped of periosteum; and I have taken no notice of that occurrence, and yet good results have followed.

When the operation was finished, I had the limb put up in the usual plaster-of-Paris splint. This day week, being nearly eight weeks after the application of the splint, it was taken off for the first time, and I am happy to tell you that the union was found to be perfect. The shape of the limb also is perfect; and I hope, before the boy returns to the country, to be able to present the Society with a cast of the limb in its new position. It is a very satisfactory case, because I think it shows that the operation of excision of the knee-joint, when undertaken in selected cases, and when the excised limb is put up in an immovable apparatus, which need not be disturbed during the whole process of the treatment, is shorn of a great many of the dangers that are very frequently attendant upon it; and I believe that we shall be able to restore to this boy a good, shapely, useful limb, instead of his being a cripple, as he was when he came under my care.—*December* 9, 1876.

Fracture of the Skull.—DR. BARTON said : The case which I beg leave to lay before the Society is one of injury to the head, possessing several interesting features. A tradesman, aged forty-seven years, was, on the evening of the 17th November last, about nine o'clock in the evening, in the Marshalsea Barracks, where he had come to see one of the sergeants. Not finding his friend in, he joined some persons in drinking, and, after a couple of hours or so, went across the barrack-yard for his wife, who was also spending the evening with a friend. The man was at this time drunk, according to the statement of a sergeant, who afterwards gave us information on the subject. Having passed across the barrack-yard, he proceeded to micturate. He micturated into an area, but, while buttoning up his trousers, stumbled forward, and fell headlong into the area, which was from four to six feet deep, and from three to four feet wide. A policeman, who found him in the bottom of the area, reported that he was then so far sensible as to know what he was doing, and to be able to bid him good night. He was brought in a cab to his own residence. He was able to stand on his feet in a kind of a way, appearing to be more under the influence of drink than much injured by the fall. He did not vomit, or bleed from the ears or nose. His wife put him to bed. He got up during the night, and appeared not to know where he was. He was able to get out of bed ; and he talked to his wife, but not in a very sensible or coherent manner. At the time of his admission into the hospital, he was in a semi-comatose condition. That was about twenty hours after the accident, on the following day. He was then quite able to walk; and, after having been put to bed, he was continually struggling to get up out of it. He succeeded in doing so, and walked down the length of the ward. When spoken to he answered in an incoherent manner. Afterwards he was able to understand, to a certain extent, what was going on, but lay, for the most part, in a semi-

comatose condition. On examination of his head I found a scalp-wound over the right parietal bone. The wound was superficial, and the skin appeared as if it had been recently bruised, but I could not feel any depression or other sign of a fracture on the region of the bruise. The pupils acted slowly on being exposed to light. His pulse was 80, and he was then able to swallow well, and even to walk. Four grains of calomel were placed on his tongue. He was restless duing the night. His bowels were moved in the morning. He still continued in the same semi-comatose condition. I ordered him small doses of aconite every hour. His head was shaved, and a blister was put over his head from ear to ear. His pulse was 92, and in the evening 86. He took beef-tea, and pushed away the cup when he had had enough. His right arm was found to be flexed rigidly. There was a rigid contraction of the muscles of the right hand, which, when opened out, immediately sprung back into the flexed condition. He then swallowed fluids with difficulty, and with reluctance also. His pulse was 96; his respirations 32; his temperature 99·8°. Three leeches were applied to his mastoid process. He became very restless, and was continually trying to get out of bed. Fifteen grains of bromide of potassium were given to him every third hour. At four o'clock on the fourth day after his admission, he had an epileptic fit; and, at seven o'clock p.m. on the same day, he had another fit of the same kind, which lasted five minutes. On the 22nd he slept well, having had one slight fit that morning. His right side and right arm were now completely paralysed, although he had some slight power in the right leg. He had received a wound in the left wrist—a jagged tear, and red lines spreading from it ran up his arm, marking the course of the lymphatics. He now lay on his back, staring and gaping at objects around him, picking the bedclothes, and much less conscious than he had been before. His pulse was 100 and his temperature 101°. A drachm of mercurial ointment was ordered to be rubbed into the axilla. He had several epileptiform fits during the evening. During each fit he regained power over his paralysed arm, his pulse being 120, and his respirations 38. On the morning of the 23rd, he was found to have had thirty-nine fits since the previous evening. He took no nourishment during the night. His pulse was now 140. Not being able to swallow, he got enemata of beef-tea. These enemata were continued twice a day, and had an excellent effect in supporting him. On the 23rd, being a week after his admission, his gums became sore, and the mercurial fœtor from his breath disagreeable. On the 24th his pulse was 100, his skin cool, his face not so flushed; he had had no fit since twelve o'clock the preceding day, and looked better and more lively. When approached by any person, he turned his head and looked, and, if spoken to, answered questions—that is, to a certain extent, he showed intelligence, and replied partially. On the 25th his pulse was 100. The mercurial ointment was

repeated. On the 26th the skin over the sacrum became very sore. He was turned on his right side, and went on gradually improving until about the 30th, upon which day the note taken was:—"He is much clearer in his mind; answers questions freely—is better after each time the nourishing enema is given." He had at this time recovered perfectly the power of his right arm and right leg. The fits did not return, and he partook freely of nourishment, consisting not merely of fluid but of bread and other solid food of that kind in his beef-tea and milk. He knew his wife, and spoke to her, and laughed and joked a little. Still he was but partially sensible of what was said to him. He answered, at times, very discreetly, but would immediately wander away again. On the 5th of December we found that, although his intelligence seemed to be improving, aphasia was taking place—that is, he would answer one or two questions very well, and would then go on repeating one word which he seemed to think conveyed his meaning, but which was really unmeaning. For instance, when asked "how do you do?" he would say that he was much better, and would then say "syme, syme," or some other unmeaning sound of that kind. On the 6th he had still some amount of sensibility, but kept groaning very heavily. He was nourished with enemata, which were taken freely. The mercurial inunctions had been kept up, and his mouth was still sore. Fresh blisters were ordered at the back of his ears. On the 7th instant, the day before yesterday, his breathing rather suddenly became very hard, his pulse rose to 150, and he died. *Post mortem.*—On an examination of the cranium I discovered the following facts:—On raising the scalp I found over the left side of the head, under the occipito-frontalis, and between it and the pericranium, a considerable effusion of blood. On raising the calvarium, I found a very extensive fracture, radiating from the parietal eminence on the *left* side; but although the fracture was on the left side, it was upon the right parietal eminence that the wound existed. The force that caused the fracture evidently took effect on the left frontal bone—because, not only does the fracture radiate in two directions from it, but we perceive a third crack extending forwards from the same spot. Tracing the fracture, we find that, after passing across the parietal bone, it comes into the occipital and crosses to the parietal on the opposite side. It crosses the squamous portion of the temporal bone, and, at the base of the skull, passes slightly forward to the great wing of the sphenoid, but cannot be traced further towards the base. On raising the calvarium, I found a rather limited effusion of blood between the dura mater and the bone. It was about the size of a five-shilling piece, partly discoloured, and adherent to the bone. Looking from the inside, we see the fracture runs down through the groove of the posterior branch of the middle meningeal artery; but that any effusion of blood could have come from it is very doubtful, for the external effusion of blood on the dura mater

was very limited. On opening the dura mater we found a very extensive extravasation of blood in the cavity of the arachnoid. It is spread out in a thick layer all over the hemisphere, but finds its greatest concentration in the middle fossa of the skull, where there is such a clot as to press upon the middle lobe of the brain very much. We also see extreme vascularity of the brain substance. The whole of the anterior part of this left lobe is softened and broken down. Here there is a mass of blood again, and the brain substance is softened, so as to be readily washed away, and it is in an active state of red softening. The middle lobe is in a similar condition. The fissure that lies between the two is in the very centre of the part of the brain where inflammatory action took place. This case, in its course and in its termination, has two or three points of interest. In the first place it bears on the diagnosis of these injuries. It is a point of difficulty, no doubt, in all such cases to tell the exact nature of the injury on which the symptoms depend. In this case the only external wound we had to guide us was directly on the right parietal eminence, whereas the fracture was on the opposite side; and, after careful consideration, we decided that this was no sufficient guide to enable us to operate or to afford us any assurance that our doing so would relieve the symptoms. The *post mortem* shows how wide this extensive wound was from the real seat of the injury. Again, the position of it suggests another thing. Had this man recovered, which he very nearly did, we might have said, "Here is a case in which the injury is on the right side, and the paralysis on the same side." But we see that would have been without any foundation whatever. Before leaving the subject of diagnosis, I venture to say that I thought the group of symptoms, during the progress of the case, was such as to enable one to give the opinion, at least, that there were here laceration and contusion of the brain substance. I think the anterior and middle lobes, which we see now softened and broken, were, in the first instance, the seat of injury, the exact nature of which is discovered by the subsequent inflammatory changes which have taken place. The symptoms that have been specially mentioned, as indicating brain contusion, are—clonic spasms of the muscles and excessive restlessness ; and these were certainly present in this case. Again, the epileptic symptoms seem to have been coincident with the establishment of inflammatory action, and the subsequent decline of these symptoms, when the mercury had produced its specific effect on the system, confirms this opinion. With respect to the immediate cause of death, I think we have it in this destructive inflammation of the lower part of the middle and anterior lobes, in connexion with which the special symptom of aphasia was present during the last few days.—*December* 9, 1876.

TRANSACTIONS OF THE CORK MEDICO-CHIRURGICAL SOCIETY.

Rupture of Uterus. By Dr. D. C. O'Connor, Jun.

On the 5th of October last, while doing dispensary duty as *locum tenens*, I was sent for to attend a woman reported to be in labour. On arriving at the house about twenty minutes to seven o'clock, I found that the membranes had ruptured about four o'clock, and on making a vaginal examination found the os nearly dilated, a head presenting; the parts were all soft, moist, and natural, and everything apparently favourable. On inquiry I learnt from the nurse, a very intelligent woman, that the patient had previously borne thirteen children. The last three of her confinements were very tedious and painful, the woman suffering very much, but not requiring medical attendance—this same midwife officiating in each. Remaining in the next room I made occasional examinations, and everything seemed progressing favourably, the os becoming almost fully dilated, the head descending gradually, the pains were regular but not showing any abnormal force, indeed rather the contrary. A little before eight o'clock I heard the woman complain of a pain in her side; again hearing the same complaint, I questioned her and found that she had apparently a severe pain in the left side of the abdomen, which was quite localised, continuous, and independent of the labour-pains. Staying by the bed-side for some time I found this pain to increase, the labour-pains seemingly decreasing in force, and a longer interval between each; the pulse fluttering; the head was fairly down, but had made no progress for about half an hour. Beginning to feel a little uneasy, and not having forceps with me, at half-past eight I sent a messenger to Dr. Holmes, asking his assistance. From this time I noticed an entire cessation of the labour-pains, the face quickly assuming a pallid hue, and the pulse suddenly sinking. Dr. Holmes arriving just at this time, I communicated to him my fear that the uterus had given way; he making an examination, found the head had receded almost out of reach, and it was quite evident that a rupture had taken place. We tried then to apply the forceps, but at the first attempt a large quantity of blood gushed forth, and we then found that the fœtus had entirely disappeared into the abdominal cavity.

The woman being then almost at the point of death, and any hope of saving the child past, we deemed it better to leave her to the ministry of the clergyman.   In about a quarter of an hour I returned and found her dying, death taking place almost immediately.

The next day, Dr. O'Sullivan and Dr. Atkins kindly accompanying me, I returned to the house, having represented to the nurse the necessity of removing the child.   Dr. Atkins making a section of the abdominal wall, we found the following result :—The cavity was filled with blood, and the umbilical cord was visible, on tracing which was found, embedded in the intestines, a full-grown healthy-looking male child, lying back uppermost, head towards the pubis.   On examining the uterus we found a rent at the posterior surface, extending from the left side of the fundus down to the junction with the vagina.   The anterior wall of the uterus was very thick, being at least from two to two and a half inches.   The posterior walls being not more than about two lines in depth, and its inner surface being quite frayed away, showing abundant evidence of old-standing ulcerative endometritis.   The placenta, I might mention, was attached to the anterior surface and easily detached.

On inquiry I found that the woman had married at fifteen, being but thirty-five at the time of her death, and, as I said before, had thirteen previous confinements.   She had, I understood, lived in a house of ill-fame, in the capacity of housekeeper, and had suffered considerable ill-usage at the hands of her husband.   She complained for some two years previously of this same pain in the left abdominal region, and had frequent hæmorrhagic discharges.

Assisted by my friend, Dr. Atkins, I made a histological examination of different surfaces of the uterus, previously hardened in chromic acid. A section of the anterior surface showed the fibres to be somewhat separated, the open mouths of the vessels dilated, and the entire surface of the section covered with minute, irregular-shaped, and dark-coloured granules, more resembling pigment than anything else.   What the origin of these granules may be, or whether it is a truly pathological condition, I am unable to say.

A section of the posterior surface near the seat of rupture showed the fibres to be softened and easily separated, as can be seen in the specimen exhibited for your inspection.   The uterine arteries were very widely dilated and their walls thickened.   The centre of the section was found to be occupied by an almost organised blood-clot, the fibres surrounding it in a horse-shoe form ; somewhat similar granules, but in smaller quantities, were also observed.

At seat of rupture a section showed several clots infiltrating the tissue. The prominent features throughout are the separation and softening of the fibres, which is, in part, due to the normal parturient state of the organ.   The truly morbid condition observed is the localised and separ-

ated extravasation, forming loculi, as seen in last mentioned section, several of the loculi being very minute. It is to be regretted that owing to the extremely friable nature of the tissue, the actual ulcerated surface could not be submitted to accurate microscopic investigation. The broken fibres seen in the second section may be due to the chronic inflammatory condition of the tissues. I regret that the last process of mounting the specimen was nearly fatal, the varnish penetrating the mounting medium.

One point to which I should invite your attention is, whether there would have been any chance of saving the woman's life if the forceps had been applied earlier. My first idea was that the woman's life might have been saved. My second, and this was shared by my father, that judging from the extremely thin and frayed condition of the posterior wall, death would have resulted in any case, either from rupture or hæmorrhage, and the use of the forceps get the credit of the fatal result.— *November* 8, 1876.

SYPHILITIC EPILEPSY.

DR. ALFRED FOURNIER published in *L'Union Médicale* during the past year an interesting series of lectures on tertiary syphilitic epilepsy, which contain pretty nearly a complete summary of all that is at present known in regard to the subject. We only extract the final practical conclusions in reference to the diagnosis and treatment of doubtful cases of epilepsy. M. Fournier says:—1. Every case of epilepsy appearing for the first time in an adult subject of known syphilitic antecedents should be attacked with specific treatment. 2. Every case of epilepsy happening under the same conditions in a person of uncertain or denied syphilitic antecedents should still at least, if not plainly explicable by some other cause, be submitted empirically to the specific treatment. In obeying these two precepts marvellous successes may be sometimes obtained (*American Journal of Nervous and Mental Disease*). In a recent lecture on this subject, M. Charcot lays considerable stress, as an aiding point in diagnosis, though not a characteristic sign of the disease, on a fixed and localised cephalalgia preceding for a long time the convulsive attacks. Dr. Buzzard has also noticed this pain as associated with syphilitic epileptic seizures. M. Charcot recommends in some cases the inunction of mercurial ointment, in quantities varying from a drachm and a quarter to a drachm and a half daily, and the administration at the same time of iodide of potassium in doses of from one and a half to two, or two and a half drachms in the twenty-four hours, partly by the mouth and partly by enema.

R. A.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, February 24, 1877.

Towns	Population in 1871	Births Registered	Deaths Registered	Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhœa	Annual Rate of Mortality per 1,000 Inhabitants
				DEATHS FROM ZYMOTIC DISEASES							
Dublin, -	314,666	696	673	9	7	9	3	3	26	12	27·8
Belfast, -	182,082	541	449	—	40	8	4	26	10	12	32·0
Cork, -	91,965	254	180	—	—	—	5	—	5	6	25·3
Limerick, -	44,209	92	89	—	2	2	2	1	1	4	26·3
Derry, -	30,884	59	71	—	—	1	—	3	—	—	30·0
Waterford, -	30,626	77	49	—	—	—	—	—	—	—	21·0
Galway, -	19,692	32	46	—	—	—	—	3	—	1	30·3
Sligo, -	17,285	26	33	—	—	—	—	—	2	1	24·8

Remarks.

In Belfast, Galway, Derry, and Dublin, the rate of mortality was very high; in Limerick, Cork, and Sligo, it was high; and in Waterford it was moderately low. The death-rate per 1,000 of the population annually was 22·1 in London, 19·5 in Edinburgh, and 25·8 in Glasgow. Omitting the deaths of persons admitted into public institutions from localities outside the registration district, the rate in Dublin was 26·6 per 1,000. Only 91 deaths from zymotics were registered in Dublin, compared with 123, 114, and 120 in the three preceding four-week periods. Small-pox was more fatal than before. The zymotic death-rate in Belfast remains inordinately high—measles and whooping-cough are raging as epidemics. Of the 26 fever-deaths registered in Dublin, 7 were caused by typhus, 12 by enteric, and 7 by simple continued fever. Small-pox was not quite so fatal in London as in the preceding period—it caused 369 deaths against 381 in the first four weeks of the year. The continued defective registration in the South Dublin Union Workhouse is matter for sincere regret. The Registrar-General remarks, under date February 26, 1877, " Of the 21 deaths registered from the South Dublin Union Workhouse, in 13 cases the cause of death was returned as

'unknown.' The Deputy-Registrar states that the chief resident officer informed him he was unable to state the cause of death, as the medical officers had not sent in any certificates of cause of death in these 13 instances."

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20′ N., Long. 6° 15′ W., for the Month of February, 1877.

Mean Height of Barometer, - - -	29·930 inches.
Maximal Height of Barometer (on 28th at 9 p.m.),	30·390 ,,
Minimal Height of Barometer (on 25th at 9 p.m.),	29·297 ,,
Mean Dry-bulb Temperature, - - -	44·0°
Mean Wet-bulb Temperature, - - -	41·8°
Mean Dew-point Temperature, - - -	39·1°
Mean Elastic Force (Tension) of Aqueous Vapour,	·240 inch.
Mean Humidity, - - - -	82·5 per cent.
Highest Temperature in Shade (on 15th), -	57·2°
Lowest Temperature in Shade (on 28th), -	27·4°
Lowest Temperature on Grass (Radiation) (on 28th),	24·5°
Mean Amount of Cloud, - - -	64·0 per cent.
Rainfall (on 19 days), - - - -	1·560 inches.
General Direction of Wind, - - -	W. and N.W.

Remarks.

The open weather, which has been so characteristic of the present winter, continued uninterruptedly until the 15th, on which day the shade thermometer rose to 57·2° about 2 p.m.; an hour later the wind veered from S.W. to W. in a strong squall, with heavy rain, and by 9 p.m. the temperature had fallen 17·5°. On Sunday, the 18th, a cold period set in, and continued until the following Friday. It was caused by a change in the distribution of atmospherical pressure in N.W. Europe. The area of high barometer became established in the S.W. of Ireland, instead of being over Spain and the S. of France, and a series of cyclonic systems travelled from N.W. to S.E. across the British Islands and Scandinavia. Accordingly, polar winds prevailed, with cold searching weather and showers of hail, rain, and snow. After two days mild weather, on the 24th and 25th, a polar air-current again descended on the United Kingdom, and the severest frost of the season occurred on the 28th. The heaviest rainfall in twenty-four hours was ·421 inch on the 13th. Snow or sleet fell on the 20th, 26th, and 27th. Hail fell on the 2nd, 3rd, 20th, 22nd, 26th, and 27th.. A lunar halo was seen at 10 p.m. of the 26th. The total eclipse of the moon on the evening of the 27th was seen throughout under exceptionally favourable circumstances.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

ON THE INFLUENCE OF BLOOD-PRESSURE UPON THE RHYTHM OF THE HEART.

DR. S. TSCHIRIEW, of St. Petersburg, has made some experiments on this subject, which will confirm the discovered facts of Ludvig, Thiry, Bezold, and Cyon, as having brought to light some new features with regard to the rhythm of the heart. In particular he noticed the very considerable and sudden retardation of the beats of the heart by increasing the blood-pressure by merely severing the nerves in the neck; and also, after complete separation of the heart from the central nervous system had been executed, he observed a subsequent quickening of the pulse which could be induced by compressing the abdominal aorta. His investigations led him to the following conclusions:—(1) Considerable and rapid fluctuations of the blood-pressure can change the rhythm of an isolated heart. (2) Each strong and rapid fluctuation is able to stimulate directly both the apparatus which stops the heart, and also the motor ganglia of the heart, so that the beat is either increased or diminished, or, more rarely, it remains unaltered. (3) The concluding character of the changes in the heart-rhythm during the time of increased blood-pressure depends on the alternate influence of both excitations. Since a weak irritation of the vagus is sufficient to suppress totally the expression of the accelerating nerve (Bowditch), so it is easy to understand why (4), in most cases where the heart-stopping apparatus is sufficiently developed and excitable, the increase of the blood-pressure in most instances retards the heart-rhythm, and then, on the cessation of the blood-pressure, the accumulated excitation of the motor ganglia comes into play and thereby results a succeeding acceleration. If, on the contrary, the heart-stopping apparatus be weakly developed and it become fatigued by previous excitations, then commences at times a very considerable acceleration of the pulse even during the period of increased blood-pressure. (5) The more the acceleration of the heart-beat was present, even during the increase of the pressure, so much the less will be the subsequent accelerated action, and *vice versâ*. (6) The heart receives likewise from the accelerating nervous system a constant tonic stimulation. The central terminations of this system can also be stimulated by an increase, but not by a decrease, of the blood-pressure. (7) In the normal condition there is joined to this direct influence an indirect one by the vagus and accelerating nerves. (8) Small doses of atropia paralyse the

peripheral ends of the vagus nerves, but not the heart-stopping apparatus itself. (9) The "pulsus bigeminus" is merely a retarded pulse, while the cavity of the heart is being peristaltically contracted. (10) Anacrotism of the pulse, as is the case, for instance, in insufficiency of the aortic valves and sclerosis of the arteries, is the expression of peristaltic contraction of the heart, not the elastic fluctuation of the walls of the vessels.—*Centralblatt*, No. 35, 1876.

<div align="right">J. M. F.</div>

ON THE METHODS OF ELIMINATION AND ON THE ELECTIVE ACTION OF QUININE.

UNDER this title Drs. Albertoni and Ciotto contribute the details of certain experiments made by themselves to the *Bull. Gén. de Thérap.*, Nos. 8 and 9, 1876. They show—1. That the presence of quinine in the bile may be demonstrated in from two to five hours after its introduction into the stomach. 2. The elimination of quinine by this path is quite active, since it is found only two hours after ingestion, and after the ingestion of sixty centigrammes (9 grains) only. It is found also that in the dog it produces, in twenty-four hours, an average secretion of 400 grammes of bile. 3. Though quinine may not be found in the bile, sometimes until two and a half hours after ingestion by the mouth, this is not against the fact that its proper route for elimination is through the hepatic secretion, since, as it is known, this is much less rapid than elimination by the urine. To MM. Albertoni and Ciotto the positive result that the biliary secretion contains quinine administered by the mouth appears important. The common mode of entrance of quinine, as of almost all medicines, is by the digestive tube. In the stomach its absorption is favoured by the acids present, particularly by the hydrochloric acid, while in the intestine, on the contrary, this is rendered less easy by the alkalinity of the enteric and pancreatic secretions, and still less by the biliary acids which form insoluble combinations with quinine, though these last are soluble in excess of acid. Thus the absorption of quinine in the intestine only takes place sparingly under physiological conditions. Once entered by gastro-enteric absorption into the portal circulation, quinine finds a natural anatomico-physiological route for elimination in the biliary secretion. This fact the experiments of MM. Albertoni and Ciotto *positively* demonstrated. The presence of quinine in the biliary secretion serves to establish the important fact of its *electivity of action*, since it shows how this alkaloid places itself in intimate contact with the hepatic cells, constituting the functionating element of the liver, of which the bile is the complex product of secretory elaboration. Quinine, therefore, introduced by alimentary passages, appears to stop by preference in the liver and in the spleen. As regards the question whether quinine introduced into the circulation by other routes than by the portal system is

eliminated by the bile or by the urine alone, Messrs. Albertoni and Ciotto find that, hypodermically injected, quinine is eliminated by the urine, an important fact in practice, for it is thus useless to administer the remedy by this method if we expect to affect the liver and spleen. Quinine, taken by the mouth, is in part eliminated directly by the portal circulation without passing into the general venous system. As regards the length of time during which quinine remains in the organism, it has been found in the urine sixty-eight hours after ingestion. Finally, quinine was always found by MM. Albertoni and Ciotti in the spleen, nearly always in the liver, viscera in which it remains for the longest time. In the heart, quinine is found in larger quantity when introduced hypodermically than when taken by the mouth. In the brain it appears very quickly, but in smaller quantity than in the other viscera mentioned.—*Phil. Med. Times.*

PEROXIDE OF HYDROGEN IN THE PREVENTION AND TREATMENT OF SCARLET FEVER, &C.

Dr. John Day, of Geelong, Australia, as most of our readers are probably aware, has great faith, and apparently justly, in the disinfecting properties of peroxide of hydrogen. It appears to be an agent specially suited for the destruction of the poison-germs of scarlet fever, small-pox, and other epidemic diseases. This is due to its containing a larger amount of oxygen than any other known substance. One-half of its oxygen is loosely combined and in a highly active condition, ready to combine with any organic matter with which it may be brought in contact. Consequently it is reasonable to infer that by coating the body of a person suffering from scarlet fever or small-pox—diseases in which most of the poison is eliminated by the skin—with peroxide of hydrogen in combination with lard, cacao butter, cold cream, or any other substance which will conveniently retain it, we are reducing the danger of infection to minimum. The ethereal solution of peroxide of hydrogen is commonly but erroneously called ozonic ether. Dr. Day's latest formula for its external application in scarlet fever or small-pox is :—Ozonic ether, four drachms; pure lard, four ounces; benzoic acid, twenty grains; otto of roses, four drops : to be carefully mixed without the aid of heat. The benzoic acid, in addition to being a powerful antiseptic, possesses the property of allaying cutaneous irritation. A gargle composed of two drachms of ozonic ether in eight ounces of water benefits the throat symptoms. Dr. Day's results in checking the extension of scarlet fever and in its treatment by this method have been very satisfactory.—*Med. Times and Gaz.*, March 10.

LARYNGEAL PHTHISIS.

CHAMBERLAIN remarks (*Proceedings of Connecticut Medical Society,* 1876) that the term laryngeal phthisis, formerly used to include all chronic affections of the larynx, involving ulcerative or destructive changes of its tissues, especially the cartilages, has been shown, since the introduction of the laryngoscope, to be of a much more limited applicability, and that it is not perhaps strictly correct as implying a separate and independent disease, rather than a local manifestation or complication of a general condition. He uses it, therefore, to include all the affections of the throat associated with pulmonary consumption, the same general conditions underlying the changes in the larynx or lung, the dyscrasia striking now at one organ, now the other. A short *résumé* of the pathological nature of tubercle brings him at once to the disputed question. Whether tubercular deposits occur at all in the larynx, or, if so, whether they play an important *rôle* in the production of the laryngeal lesions, or are these all adequately explained by catarrhal or follicular inflammation and ulceration? The answers to this question are very conflicting, and the views concerning the relation of tuberculosis to the lesions in the larynx vary pretty uniformly with the author's opinions concerning the nature of the changes in the lungs, although, strangely enough, Louis, finding no evidence of miliary tubercle in the larynx in any of the large number of cases he examined, attributed the ulcerations to inflammation, which might be caused by the sputa excoriating the mucous membrane. Similar negative testimony is given by Alison, Rühle, and others; on the other hand, Cruveilhier decided the lesions to be non-tubercular from the absence of caseous degeneration. Andral, Laennec, Trousseau, and others of the older writers, considered the ulcerations which occur late in phthisis as undeniably of a tubercular nature, and Hasse carefully demonstrated the similarity of the anatomical elements, which the microscope revealed, to others which were admitted to be tubercular. The views first promulgated by Rheiner have had a very wide acceptance, and undoubtedly are correct in a certain number of cases, while others are incapable of satisfactory explanation in any such way. He describes a catarrhal or follicular inflammation followed by ulceration, attended with a thick cellular infiltration of the mucous and submucous tissues, produced by a rapid multiplication of already-existing elements, the thickened margin of the ulcers caused by serous infiltration (Lebert, " Anatomie Pathologique," p. 594). Mackenzie modifies this by assuming imperfect cellular elements, and a special constitutional condition inherited or acquired. Rindfleisch accepted substantially this view, admitting, however, the presence of tubercular granulations in the larynx, occurring especially where two surfaces are rubbed together; but he considered that they acted only as a permanent irritant, the changes

2 D

being due mainly to inflammation, although in Ziemssen's Cyclopædia he describes a tuberculisation of the larynx which is secondary to catarrhal inflammation, and occurs in scrofulous subjects. Rokitansky and Virchow assign a much more important place to tubercle as a cause of the pathological changes in the larynx; the latter, indeed, recommends this as the best place to study true tubercle, and explains much of the negative testimony by the fact that the granulations so soon break down into shallow ulcerations. Jaccoud ("Anatomie Pathologique"), whose classification is followed essentially by many of the modern French writers (Fauvel, Mandl, Thaon, Eugene, and Boeckel), describes a primary tuberculosis of the larynx with discrete tubercles, and a laryngitis of the tuberculous form with infiltrated tubercle. Isambert qualifies this by denying that the two varieties can be separated by any sharply-cut line, still he admits the existence of both classes of cases. Wahlberg describes the characetristic appearances of tuberculous ulcers and deposits in the larynx, after a thorough and careful study, and illustrates their minute anatomy in a series of beautifully-executed microscopical drawings. The same elements that characterise tubercular deposits elsewhere are found here; a granular centre surrounded with cellular infiltration, the round lymphoid cells, giant-cells, and indifferent cells, all embedded in a reticulated network; this infiltration occurs alike around the nodules which have not yet broken down into ulceration, and around the margins and base of the ulcers. The epithelium over the nodules is changed from the normal ciliated variety into pavement epithelium. The network of capillaries which surrounds the ducts and their glands explains the more frequent occurrence of cellular infiltrations around them, as this is produced by wandering cells from the blood-vessels. Tubercular nodules are also found where the glands and their follicles are still normal, thus excluding any invariable connexion with catarrhal or follicular inflammation (Stricker's "Medicinische Jahrbücher, 1872). Chamberlain, personally, considers the affections of the larynx accompanying phthisis as divisible into two classes:—1. The tubercular, characterised by the presence of tubercles, either primarily or secondarily; and, 2. The non-tubercular, including those which present no evidence of the presence of tubercles at any stage of their progress, but which are, nevertheless, decidedly characteristic. A description of the subjective and objective symptoms of the first class of cases follows, which is excellent, and well worth careful study. The earliest indications of implication of the larynx, constituting what might be called a pre-tubercular stage, as it is almost pathognomonic, are a marked and general anæmic condition of the membrane of the larynx, sometimes extending to the pharynx, soft palate, and uvula, and, if the similar appearance presented in cases of general anæmia, and sometimes accompanying uterine derangements, be differentiated, the condition is of marked value from a diagnostic point of

view. The aphonia, which also occurs as a symptom of incipient tubercular disease in the lungs, without any implication of the larynx, is apparently caused by a reflex irritation of the laryngeal nerves, due to disturbance of the peripheral branches of the pneumogastric by the tubercular processes in the lungs; the tensor muscles are the ones affected, and here a differential point in diagnosis is given between this form of aphonia and that due to hysteria, where the adductor muscles are at fault. Following the stage of anæmia, there occurs a deposition of yellowish nodules, about the size of a pin's-head, with a glistening surface, most abundant in the upper and posterior portions of the larynx. Some congestion and tumefaction occur, which increase as the nodules break down into ulcerations, but never become marked. These ulcerations are irregularly circular in outline, usually multiple, exhibit a marked tendency to spread and unite with adjacent ulcerations, and are covered with a muco-purulent secretion. If they continue, they involve the deeper tissues, and then congestion and tumefaction become more marked. The second class of cases are characterised by great tumefaction and congestion, without any evidence of tubercular deposit or ulceration. Sometimes the epiglottis is principally involved, at other times the tumefaction is confined to one side of the laryngeal opening. In scrofulous cases there is often a similar swelling of the epiglottis and upper portions of the larynx—this, however, seems to be caused by a thickening of the sub-mucous fibrous tissue; these cases are of long duration, and generally end with secondary tubercular ulceration; the membrane of the larynx, on inspection, is pallid and almost white, but differs from anæmia in its glistening appearance, and the tumefaction that accompanies. This sub-mucous fibrous proliferation also occurs in syphilis, and is occasionally fatal from obstruction; the diagnosis sometimes is difficult, as mercurials which might aid to establish it are contra-indicated in phthisis. Cancerous affections are much more easily distinguished; the lesions of syphilis more closely resemble those which accompany phthisis. In other cases, after a well-marked stage of congestion and tumefaction, the latter are not so excessive as in the form just described, ulcerations occur upon the vocal cords or near their posterior insertion, also on other portions of the larynx as well as upon the epiglottis. These ulcerations involve the deeper tissues, and are sometimes accompanied with severe burning pain. They are often secondary to tuberculisation of the larynx from inflammation, and attended with grayish nodules which show distinctly through the congested membrane. The treatment should be local, general, and climatic. Direct application with the laryngeal brush of astringent and stimulating solutions, applied regularly at short intervals, with the direct spray, and inhalations of gases or medicated vapour, are serviceable. Strong caustic applications are to be avoided, and are only

exceptionally of use; as a rule they are of doubtful utility, and produce more or less discomfort. The application to the larynx, by brush, of opiate solutions, bromide of potassium in glycerine, or the insufflation of morphine in sugar of milk, and the like, often give the most decided relief. Painting the epiglottis and upper portions of the larynx with olive-oil is useful, and often renders swallowing of food possible and easy, when otherwise attended with the greatest difficulty and pain. Iodine dissolved in olive-oil, as recommended by Marcet, gr. x—xx to the ounce, can be used with benefit, even in the later stages, while externally for absorption, not counter-irritation, it is decidedly useful, especially when there is enlargement of the tracheal gland. The free use of opiates in the later stages is to be regarded as a matter of course. Chamberlain's paper is based on the histories of fifteen cases, of which nine were males and six females. The greater number were between twenty and thirty years of age; the oldest forty, youngest eighteen. There were ulcerations in the larynx and upon the epiglottis in ten cases, involving the vocal cords in five, and tubercular deposits in nine. In nearly all, the lungs gave evidence of tubercular deposits sooner or later, while in none of the fatal cases was there any favourable influence apparently exerted on the pulmonary disease by the laryngeal complication, but decidedly the reverse.—*N. Y. Med. Jour.*, Oct. 1876.

IODOFORM IN THE TREATMENT OF CHANCROIDS.

THE disagreeable odour of the drug may be obviated in great measure by using a saturated ethereal solution. This appears to be fully as efficient as the powder, and is admirable as an injection in cases of sub-preputial chancroid complicated with phimosis. The solution should be thoroughly protected from the light. The use of iodoform in the treatment of chancroids has been the common practice at the Charity Hospital, New York, for several years (*Boston Med. & Surg. Jour.*, Jan. 25). In an earlier issue of the same journal (Jan. 11), Dr. Greenhough has a good article " On the Treatment of the Chancroid." His opportunities for the study of venereal diseases in Boston are extensive, and he is convinced that, in ninety cases out of a hundred, at the very least, cauterisation, as a mode of treatment of chancroids, is not only unnecessary but injurious. Wherever possible he uses iodoform, and has had very good results. Applied either pure as a powder, or made into an ointment (ʒi. to ʒi.) it causes a chancroid to heal in a very short time in most cases, and there is a decidedly diminished tendency to the occurrence of sympathetic buboes.

PART I.

ORIGINAL COMMUNICATIONS.

ART. XIV.—*Reports in Operative Surgery.* By RICHARD G.
BUTCHER, M.R.I.A.; M.D. (*honoris causâ*) of the University of
Dublin; University Lecturer on Operative Surgery, and Surgeon
to Sir P. Dun's Hospital; Examiner on Operative and Practical
Surgery in the University of Dublin; late Senior Surgeon to
Mercer's Hospital, and Lecturer on Clinical Surgery; Honorary
M.D. of the College of Physicians of Philadelphia; ex-President
of the Royal College of Surgeons of Ireland; Fellow, Licentiate,
and Member of the Council of that Body, and for many years
President of its Surgical Court of Examiners; likewise Examiner
on Surgery, Anatomy, and Physiology in the College for many
years; Member of the Royal College of Surgeons of England;
Corresponding Member of the Surgical Society of Liège, &c.

I. ON EXCISION OF THE ELBOW-JOINT.

II. ON THE REMOVAL OF AN ENORMOUS FIBRO-CELLULAR
TUMOUR FROM THE NECK.

III. ON DOUBLE COMPLICATED HARE-LIP IN MOTHER AND
CHILD; OPERATIONS ON BOTH.

IV. ON UNUNITED FRACTURE OF THE LEG; TREATED SUCCESS-
FULLY BY DRILLING THE BONES AND DRIVING IVORY PEGS
INTO THEM.

V. ON EXTENSIVE FRACTURE OF THE BASE OF THE SKULL;
ITS TREATMENT AND RECOVERY.

IN a paper recently written by me, and published in this Journal, I
proved by additional examples the propriety of excision of the

knee-joint in suitable cases, and the practicability of saving a limb, *useful for progression and all the purposes of life, its symmetry being perfectly preserved.* I enforced this practical precept by the history of a man that I had operated on twenty years before, and illustrated his case by engravings of the portions of bone cut out, and a picture of this man when cured; and contrasted with this a picture (from a photograph) of the man after twenty years had passed by, and concluded by the remarks:—" He is now strong and vigorous, sickness has never visited him, and during this long time the limb has never failed him in all the rough usages of life through which he has passed. The countenance of the man has been marked by time, but his bodily appearance is more in symmetry than when depicted after the operation. The limb operated upon has not departed in the least from its solid, rigid position and look; but the calf of the leg is more developed, though the thigh is not more convex externally; the whole cast of the man as he stands now, twenty years having passed by, is more defiant, more at ease, and, as it were, on more perfect equality with the world. The picture seems to speak—indeed carries conviction *that, after excision of the knee-joint, the symmetry of the limb can be preserved, and also its usefulness for many many years.*" As I dealt with the knee-joint, so shall I now with the elbow-joint—detail an example of a recent case of excision, and the perfect and admirable results obtained; and next dwell upon one in which the operation was performed most successfully a long time ago—seventeen years—and its perfect condition now, after long and toilsome use of it:—

CASE I.—*Excision of the Elbow-Joint for Extensive and Incurable Caries—Perfect Recovery—the Symmetry of the Limb, with its varied and perfect motions, and full power being preserved.*—Capt. ——, an officer in a heavy dragoon regiment, consulted me, September 7, 1874, suffering most severely from a very serious condition of the left elbow-joint; he was accompanied by Surgeon-Major Wilson, of the same regiment. The young man was thirty-one years of age, six feet in height, and proportionately and powerfully built. All his life he was addicted to and excelled in athletic sports. Eight years before the above date, he for the first time sustained an injury in the joint, from a fall when out hunting; after active treatment and a fortnight's rest the inflammation subsided. Shortly after, when playing at cricket, a ball forcibly struck the same joint, giving rise to the most severe inflammatory action and high constitutional disturbances; so severe were the symptoms at this time, that two eminent surgeons proposed removing his limb. However, this proposition

was set aside, and by the most assiduous attention on the part of his medical attendant, the mischief was arrested, and the arm recovered, being partly stiffened. For a length of time it did excellent service. The limb, however, was doomed to be hurt again, for when hunting about a year ago, he was thrown from his horse and severely twisted and strained the semi-ankylosed joint; and much slow mischief followed, which was retarded by constant and careful treatment—when about six months later the arm was severely injured again, which gave rise to all the sufferings and changes which made him seek my advice on the above date.

When I saw Capt. ——, with the Surgeon-Major of his regiment, the limb was enormously swollen, and the left elbow-joint greatly augmented in bulk, as contrasted with the sound one; the patient held up the arm semi-flexed and pronated with the right hand, and seemed most anxious to steady and support it without shaking. All around the joint the integuments assumed a dark purple hue, greatly engorged with blood, and the swelling gradually fined away towards the forearm and upper arm. A small ulcerated opening lay to the inner side of the olecranon process, through which now, and for days past, escaped an abundant supply of sanious pus, sometimes of a far better character. On gently introducing a probe, I detected the disintegration of the entire joint; the humerus was, for more than an inch, rough and deprived of its cartilage of incrustation, and the ulna stripped of synovial membrane and robbed of its cartilage; the coronoid process of the ulna was likewise eaten away; the head of the radius could be discovered abraded and freed from cartilage; and, as a consequence, from the long time the arm was held in the pronated condition no crepitus could be elicited. Now the constitutional symptoms ran so high that the patient's life was imperilled—the constant restlessness and want of sleep, the persistent pain, the profuse sweatings, the total loss of appetite, the rapid small pulse—all indicated that to preserve life the source of irritation should be removed.

From the most careful examination by the probe, I could detect the localised mischief in the humerus, being chiefly confined to the joint, and a similar localised mischief of the radius and ulna; the hand was good, the fingers could be flexed, and there were no wandering sinuses. I determined to excise the joint. After expressing this opinion, I was told that a few days before I saw the patient, he went to London for advice, and that amputation was proposed as the only resource to save his life. However, Capt. —— was induced to come back to Dublin, when he placed himself under my care; and I excised the joint after the following manner:—

At 11 a.m., September 14, 1874, assisted by Surgeon-Major Wilson, Dr. Tufnell, and Dr. Wheeler, the patient was placed upon a table,

slightly rolled over on the right side, and chloroform administered by Dr. Barton; he consumed a great deal of it, and was slightly convulsed before its full anæsthetic agency was produced. I adopted the H incision, as I always prefer making the internal first stroke very carefully over the projection of the internal condyle, about four inches long, and then dissected the ulnar nerve and its surrounding coverings out of the fossa in which it was embedded, and drew all, with the internal flap, over— that is, internal to the internal condyle—parts retained so by a copper spatula. I next made the external incision over the head of the radius and external condyle, about four inches long; I next connected both by a transverse incision over the middle of the olecranon process. I then dissected freely down the lower flap to the bones, and upwards the upper flap from the olecranon and the humerus, found the end of the humerus laterally and also the ends of the radius and ulna laterally; then forcibly flexed the arm, from the semiflexed position it maintained from its former arrangement, and thrust out the end of the humerus, freeing it to above the condyles, where all was sound; next freed, in the same way, the head of the radius and the olecranon process; still more forcible flexion being made; then, with the handle of the knife and index finger, forced away the brachialis anticus and other muscles from the ends of the diseased bones in front; pressing the flexion of the limb to its full extent, the end of the humerus was made to protrude, and I passed the fine blade (reversed) of my own saw in front, just above the condyles, and cut from before backwards through hard healthy bone; in the same manner the blade of the saw was applied in front of the radius and ulna, holding back the blade well and cutting from before backwards, severing the diseased head of the radius and the entire olecranon and coronoid process of the ulna from the shaft of the bone, thus leaving the prolonged portion of the insertion of the brachialis anticus undisturbed, and the attachment of the biceps to the tubercle of the radius, and the articular ligament surrounding its neck, uncut. The sections of the bones were quite hard and healthy; ligatured four large vessels. Brushed the entire surfaces over with a solution of chloride of zinc, thirty grains to the ounce of water. I then straightened the limb, a performance which required long, gentle, steady traction; owing to the length of time it had been kept in the semi-bent position, the spastic rigidity of the muscles was very great, and overcome with much difficulty; the forearm was then bent at right angles with the arm, and the flaps were brought together and maintained so by four stitches of the wire suture in the transverse incision first, and by a similar number in each of the lateral wounds; the limb was then laid down in this position—flexed at a right angle, the forearm, midway between pronation and supination, in the box which goes by my name, and which I invariably use after resection—it is figured and described in my work on Operative Surgery, p. 186; the box was
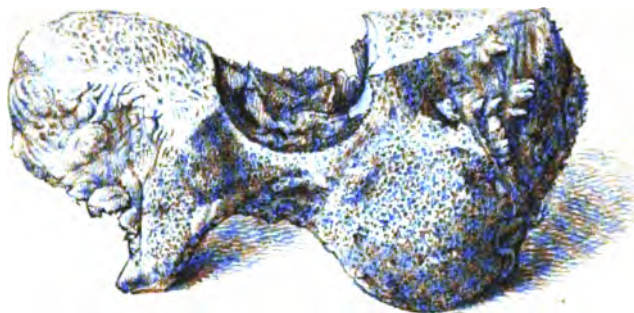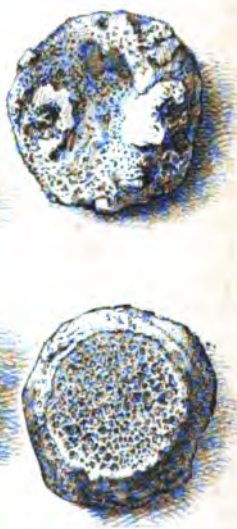
PLATE I.

Fig. 1

Fig. 2

Fig. 3

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.

ON EXCISION OF THE ELBOW JOINT.

PLATE II.
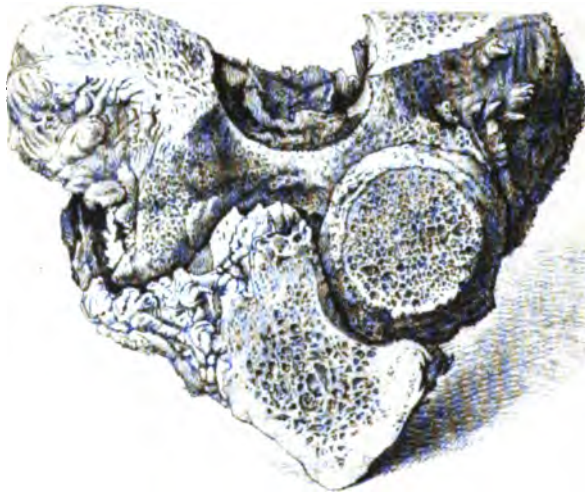
MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.

ON EXCISION OF THE ELBOW JOINT.

well padded and the limb supported everywhere when its sides were elevated; the long splint, extending from the acromion process to a couple of inches below the section of the humerus, applied and all kept steadily together by the encircling tapes and buckles; an inclined plane of pillows was laid beneath the box, and so the hand was raised considerably above the elbow, and so the returning circulation favoured. The effects of the chloroform quickly passed off; the patient was unconscious that the operation had been performed; in half an hour a full opiate was administered.

On examining the bones removed, nothing could be more satisfactory, their carious condition at once showing the necessity of the operation, and likewise the fitness of excision as applied to this case; by a reference to Plate I. it will be seen that the portions removed are represented in their full size (Fig. 1); all the cartilage is eaten away from the articular surface of the humerus, and the bone rough and carious over its whole surface (Fig. 2). The olecranon process of the ulna is likewise entirely denuded of its cartilage, and the bone eaten away, scooped out, particularly on its inner half, to a very deep extent, leaving only a thin shell of bone behind; so also (Fig. 3) the cartilage is entirely removed by ulceration from the head of the radius. The specimen altogether is a most beautiful one; the bones are unusually large, and the destructive caries most deeply marked everywhere; and the sections of the bones is most instructive, being quite hard and healthy wherever the saw had been applied. Plate II. shows the portions of bones removed in apposition—altogether three inches in length.

The patient was visited at 4 and 9 p.m. He had some quiet sleep; no bleeding from the wounds, but a good deal of watery fluid tinged with blood, such as I invariably find flows away after the chloride of zinc application; a full opiate again administered; has taken some toast and tea.

15th.—No pain; slept at intervals through the night, after taking a full opiate, and chicken-broth, also wine and water every third or fourth hour; states he has had no pain since 4 o'clock on yesterday, and the entire limb is at rest; no jerkings; no watchfulness over it during approaching sleep to guard against startings, which tortured him before the operation—in fact, he says he feels as if nothing was the matter with him, and has taken his food with appetite and desire; the limb lies quite at ease, no pain in any part of it; temperature natural; no swelling or tightness complained of. Left the limb undisturbed in its case; readjusted pillows as before, leaving the hand somewhat more elevated; watery sanguineous fluid in quantity; urine secreted in abundance; pulse down fifteen beats since the operation. To continue every fourth hour, through the night, his full sedative draught, also beef tea and wine freely.

16th.—Slept well; pulse down to 80; no sweats; no pain; feels quite comfortable; took toast and tea for breakfast; arm not disturbed. To continue opiates, wine, and beef-tea.

18th.—Going on most admirably; nothing to report on yesterday. This morning free from all annoyance; pulse down to 74, full and steady beat; no headache; no sweats; no startings; urine secreted abundantly. Let down the sides of the box carefully and in succession this morning; removed the lint placed on the wounds; the limb is quite healthy-looking; all the edges of the wounds lie evenly and glued together, very little discharge; no tension anywhere. Several pieces of lint were taken away and others adjusted in their stead, without creating the least pain; lint soaked in oil was gently and evenly pressed under the limb with a silver spatula, no wrinkles being allowed to occur; the sides of the box were then elevated, pads being adjusted exactly as before and the straps buckled; a fresh pillow was placed beneath the limb, and all steadied and supported as before, the hand being elevated slightly above the elbow. During this dressing the wounded parts were never allowed to stir, and during this manipulation the patient never complained of the slightest pain. To continue beef-tea, sherry, and opiates at night as before.

20th.—The patient going on most favourably; no pain; slept all night, and eat his breakfast with appetite. Re-dressed the wounds with same caution as before, letting down each part of the box separately and dressing the exposed surface; the wounds look in a most healthy state, healed in many parts; discharge very moderate. I pressed fresh lint soaked in oil under the limb without lifting it, and applied similar applications to the external, internal, and outer surfaces about the joint, as well as around the lower half of the arm and upper half of the forearm. The entire dressing was accomplished without the least disturbance of the divided surfaces, and with an entire exemption from pain. Opiates to be continued at night, and nutriment as before, wine in full quantity; pulse very quiet, 74, soft and compressible; urine passed in natural quantity. Ordered aperient pills at bed-time; diet unvaried. Ordered roast chicken for dinner. Ordered infusion of roses, dilute sulphuric acid, and quinine in mixture.

22nd.—Wound looks admirable, very little discharge. Re-adjusted dressings, without pain. Bowels well opened; urine in full quantity. Claret increased to four glasses, quinine mixture, and full opiates to be continued; brandy, beef-tea, and chicken.

23rd.—Slept all night; free from perspiration; tongue clean; and has taken his breakfast with great appetite. Dressed the wounds, which look most healthy; discharge very moderate; ligatures came away to-day. Re-dressed the limb as before. To continue nutriment, roast fowl, broth, claret, brandy, quinine mixture, and a full opiate at bed-time.

26th.—The transverse and external wounds firmly healed. Removed all the stitches, and applied long adhesive straps to give support; lint soaked in oil passed beneath the limb; pads and box re-adjusted. Eats and sleeps well; no perspiration, and quite free from pain. Dressed regularly every day up to this date (October 14), when I removed the box altogether, and having rolled the limb from the fingers upwards to the elbow, and the arm from above downwards to the same locality, thus making equable pressure towards the internal wound, which was not yet closed, and through which passes a few drops of matter from within, then strapped the limb corresponding to the new joint, holding the parts steadily together; then placed the limb at right angles, resting in a leather case, and steadied by a few turns of a bandage. I had the patient then removed into another room—a more bright and cheerful aspect.

October 15th.—Nothing can be more satisfactory than the state of the limb; very little discharge. Re-dressed as on yesterday, and when all dressings were off I gently flexed and extended the forearm, and then performed the motions of pronation and supination, all very easily and with but little pain; then dressed the limb, and placed beneath it the leather case, which was buckled round the patient's neck at a convenient height; no pain in the dressing or movements of the part. And now, for the first time, the patient walked about the room.

16th.—No pain; limb quiet, re-dressed, and very little discharge; bears pressure well. The patient was able to flex and extend the fore-arm considerably to-day by his own exertion and the assistance of his right hand; while without any assistance from his right hand, he could flex the forearm a little, and also extend it in a small degree; he can also open and shut the hand more than three-quarters, showing and proving that the functions of the nerves presiding over the muscles are per-fect, an exposition so anxiously looked for with regard to the ulnar nerve. The patient was allowed to get up, the limb being steadied in a case of Spark's leather, and all supported in a sling.

20th.—On this date there was such solidity about the joint, and such excellent motion, I removed the leather case altogether; re-bandaged the limb and steadied it at right angles; applied several strips of adhesive plaster outside all to give equable support; there was scarcely a tea-spoonful of discharge from the internal incision; appetite greatly improved, and sleeps the entire night without being disturbed; no sweats; getting up flesh rapidly. Allowed into the drawing-room, and permission to walk about.

26th.—Gaining every day more motion; all about the joint is quite firm, and only a few drops of matter from centre of internal incision. The patient can flex the forearm a little by the power alone of the biceps and prolonged fibres of the brachialis anticus, without the aid of the

right hand; applied over the thickened parts, around and up the arm, and over the supinators and flexors of the forearm, occasioned by the violent extension necessary immediately after the bones were excised, an ointment composed of hydriodate of potash ointment and ioduret of lead ointment, of each an ounce, with two drachms of watery extract of opium. This had a very excellent effect in removing the thickening and the dull pain which sometimes settled in the parts—a few folds of lint soaked in boiling water, and covered with oiled silk, being placed over all; limb exercised in all its motions every morning at the time of dressing, after which it was steadied with bandages and adhesive straps at less than a right angle, the entire being supported in a sling. The patient walks out now to his club, and has good use of his hand and wrist-joint.

30th.—Patient out every day, walking about, the arm supported all around, the joint well strapped and bandaged; has gained much motion in flexion and extension, also in closing hand, and in the movements of the wrist-joint; all discharges from the joint has ceased.

November 27th.—Ever since last report the patient has been gradually improving. Now the swelling has all gone; the limb has regained nearly its natural proportions. The patient, when lying down, is able to extend the arm, forearm, and hand, and having brought it to its full extent, chiefly by the action of the biceps, and without pain, to bring up the fingers to the mouth and beneath the chin. Still, however, after myself exercising the limb, I apply gradual support by bandages and adhesive straps. The young man's general health is wonderfully improved; he walks about all day, dines out, and has increased in weight, since he lay down, nearly four stone.

On December 10th, Capt. —— went home cured.

On March 20th, 1875, three months and a half later, he called on me. He could at this date raise his arm, by its flexing power, rapidly to his mouth, and make it available for all its uses, even at dinner, and for fishing, shooting, and even riding, being capable of holding the reins with it and directing the animal in all his movements.

April 3rd, 1875.—Capt. —— called to show me the perfect state of his arm. He has long since rejoined his regiment, and is equal to under-take all the active duties required by the service. On stripping Capt. —— and making him stand erect, it was most remarkable to observe the restored fulness of the whole body, the muscular development, and the scarcely appreciable shrinking of the limb that had been cut. Standing in front of the patient, scarcely any difference could be observed, either in length or bulk; and on flexing both limbs steadily at the same time, there was not the slightest hesitation or wavering in the left one. The wound marks, of course, were visible behind, but faintly, and there was no unsightly thickening anywhere, while its bulk was proportionate to the right joint. The following beautiful plates, printed by Messrs.
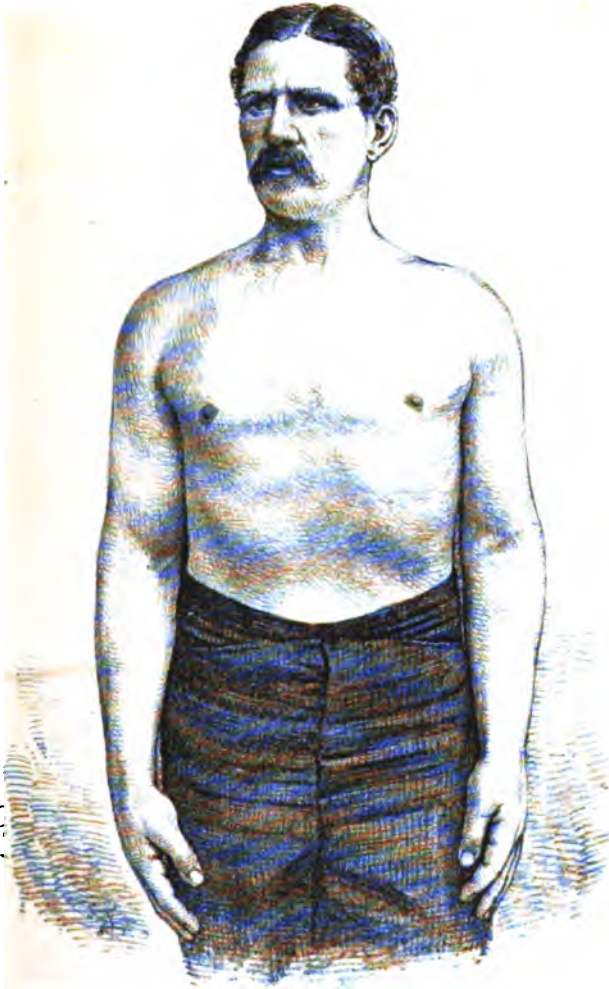
PLATE III.

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.

ON EXCISION OF THE ELBOW JOINT.

PLATE IV

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.
ON EXCISION OF THE ELBOW JOINT.

PLATE V.
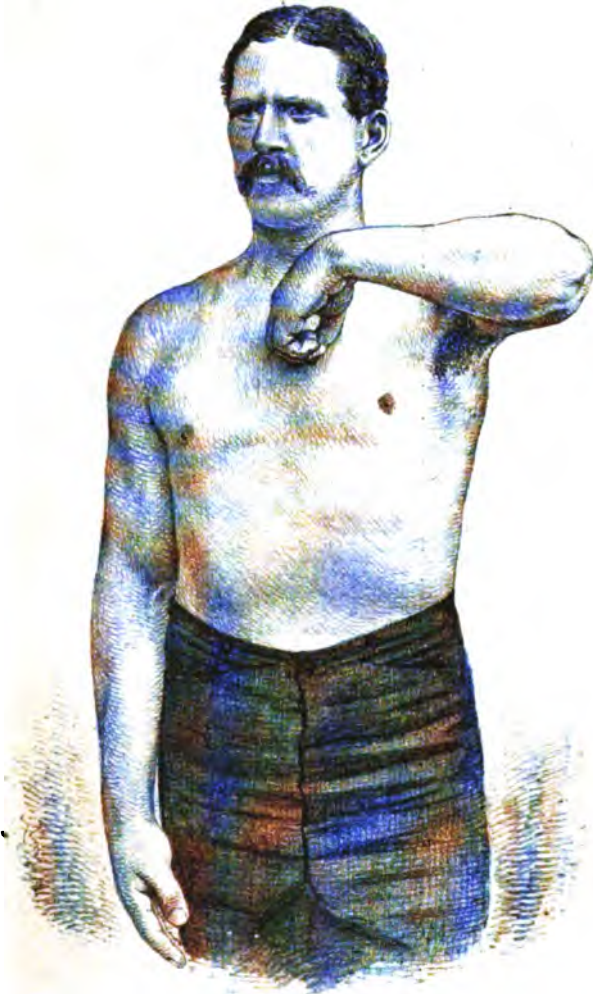
Drawn from a Photograph.   Forster & Co., Lith., Dublin

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.
ON EXCISION OF THE ELBOW JOINT.

Forster and Co., from photographs by Mr. Chancellor, taken a short time since, show, most truthfully, the perfect condition of the limb in its several aspects. Plate III. shows the healthy and well-nourished condition of the man, and also how well the length of the limb is preserved, as contrasted with the sound one, there not being more than three-quarters of an inch shortening. Plate IV. shows the arm flexed, with forearm and hand supinated and above the shoulder, revealing the angular and perfect conformation of the part from whence the joint was cut out. Plate V. shows likewise the part from whence the joint was removed, with the forearm and hand pronated. The restored power of the muscles will account for the clear distinct outlines of the arm, quite free from the slightest tremulous movement in any of those beautiful pictures—the strength and vigour of the limb was strikingly marked : the patient could catch me with his left hand and draw me forcibly from the chair and around the room, though making considerable resistance.ᵃ

January, 1876.—It is now a year and three months since the operation, and with time all the perfect functions of the limb have been restored. For months back he has been taking, as was always his custom, the most active and violent exercise, using the left arm whenever requisite, without the slightest fear as to pain, or hesitation in its action; so strong is his arm and vigorous its grasp, that he has recently been driving a coach and four spirited horses, with all his accustomed steadiness and nerve, and even through the intricate windings of a crowded racecourse.

History of the case of excision of the elbow-joint operated on seventeen years ago, and its perfect condition now, seventeen years having passed by, after long and toilsome use of it.— In July, 1859, I cut out the elbow-joint from a young man, aged twenty. His condition, when placed under my care, was as follows:—The limb was enormously enlarged, its measurement around the joint being seventeen inches, while a corresponding measurement on the sound limb was only six inches. From this enormous bulk the swelling gradually lessened above and below; for four inches the humerus participated in the bulk, and downwards for at least four inches the forearm was involved. The coloration of the part was also remarkable; and, on superficial or casual inspection, a great resemblance to malignant disease of the encephaloid form was manifest. The redness partook of a deep hue in many parts, relieved by patches of modena tint, while yellowish patches were interspersed freely. The several openings presented

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ᵃ The photographs from which the foregoing illustrations were drawn, were presented to me by the patient, and with full permission to publish them in the manner I have done.

everted edges, with fungoid spongy granulations. To the touch there was great elasticity throughout, and pitting on some parts. When the pressure was carried deeper, matter was even forced to well up from one of the many sinuous tracks beneath. The examination, in this necessary and determined way, caused great suffering; and the man guardedly steadied the forearm by grasping tightly, in the prone position, the clammy, wasted extended hand, with the sound one. The discharge from the several sinuses was profuse; through these sinuses, so extensive, so tortuous, and burrowing, curved probes were passed for many inches previous to coming into contact with dead bone; yet that the joint was broken up in its integrity, hopelessly destroyed in its adaptation, was sufficiently manifest from the unnatural mobility displayed in the slightest manipulation of the parts antero-posteriorly and laterally; the dis- integration of tissues seemed equally advanced, while from the grating within it was clear that the articular surfaces had suffered in a like way. The evidence was confirmed by the probe, and the diseased bones traced out, with much accuracy, almost throughout their entirety. I have alluded to the hand, already so characteristic, as emaciated, sweaty, and extended; however, the patient had some power in making the muscles of the forearm act upon the fingers, and ever so slightly flex and extend them, but no power could be excited in the wrist, support being taken away the hand dropped. All above the swelling the upper third of the arm and shoulder was greatly emaciated, throwing into far bolder relief the immense augmentation of bulk produced by infiltration of tissues, and new deposits all round and about the diseased articulation. Many would suppose, after inspection of the limb—aye, and even after the careful examination of it too—that amputation was more appli- cable than excision. See Plate VI., Fig. 4, for appearance of limb previous to operation (copied from Butcher's " Operative and Con- servative Surgery," Plate XVI.) So emaciated and worn had the young man become from long suffering, that he came up to town from the country to be relieved of his disease. It was quite clear that his constitution could not battle on much longer; already the fatal effects of the unrelenting irritation was becoming more and more manifest. His appetite was gone for months; he was perpe- tually bathed in sweat; his hair falling out; his pulse double its normal frequency; repeated attacks of diarrhœa; and at times loath- ing, vomiting, and rejection of all food. Here, then, we have the constitutional manifestation of the local disease sapping the very life.

PLATE.VI.

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.
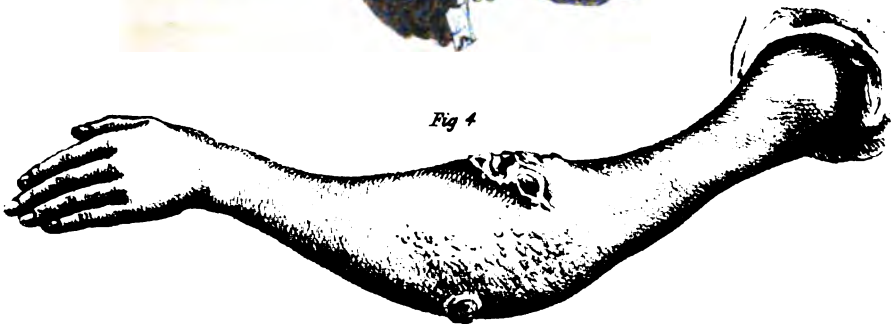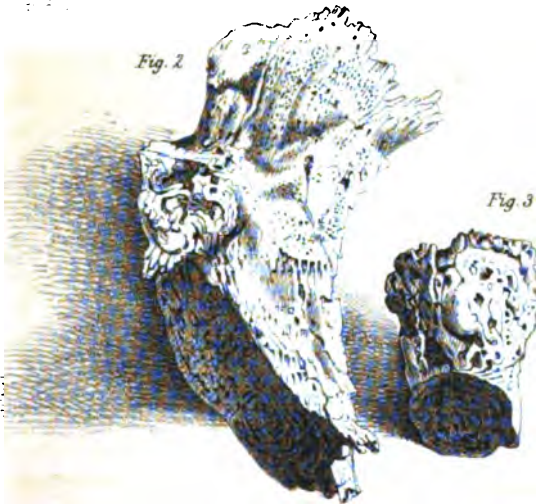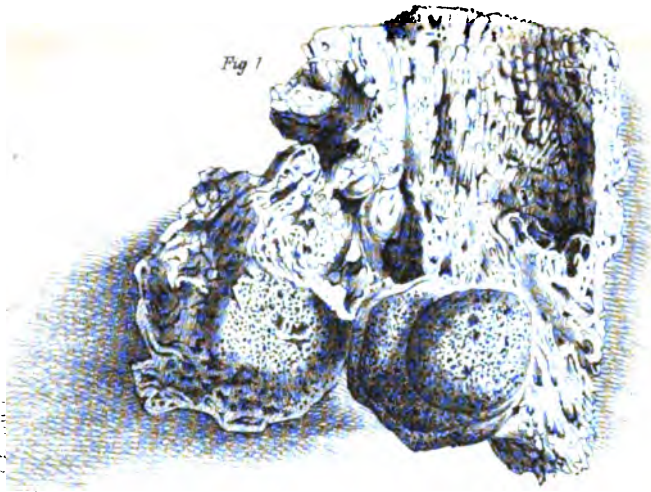
I repeat, many would have supposed the case suitable alone for amputation; however, I did not think so, from experience of serious cases of the kind, and observing the preponderating disorganisation and changes, as exhibited in the soft parts, consequent upon diseased bones. I am of opinion still, as forcibly expressed in former papers bearing upon the question of excision of joints, that much weight need not be attached to this change in the superficial structures shielding in the joint; and, as I have before written, this precept may be adduced:—"*Remove the sources of irritation, the dead bones, and in due time the soft parts will recover themselves.*"

On the 22nd July, 1859, I cut out the joint, after the manner which I have described in Capt. ——'s case. Ample and free exposition of the ends of the bones having been accomplished, " the head and neck of the radius were removed, and the olecranon process with the coronoid and an inch of the shaft of the ulna; this section included all that portion of the radius above its tubercles, and the ulna at the same time." The portions of bone removed are represented in Plate VI. (copied from my work on Operative Surgery, Plate XVII.)—Fig. 1. Lower end of humerus. Fig. 2. End of ulna. Fig. 3. End of radius. It was most pleasing to inspect their cut ends: they were hard, healthy, natural ; while the insertion of the biceps muscle was left undisturbed, a point upon which much practical importance must be placed, as preserving a salutary influence on the after-motions and perfection of the limb. The bones removed in this instance, as likewise in many others, are preserved in my private collection.

On the 1st February, 1860, the patient went to the country with the limb quite healed, and general health restored.

And, on March the 1st, had the following most favourable account from him :—All the motions of the arm steadily returning; he is able to grasp and lift weights with it, and make it useful in all its under-motions, while, in a limited degree, he is regaining the upward movements. He describes his condition as that of perfect recovery, all the motions of the limb being conducted with a steadiness and precision but little different from the sound arm; it is not yet so strong as the right one, but sufficiently so for all ordinary purposes. The conducting of this case to so happy an issue I look upon as a great triumph to conservative surgery. Certainly everyhing seemed dark and gloomy about it, so closely was the young man run down when he came under my charge; the preservation of life,

even by dismemberment by amputation, being a proposition that might stagger the most sanguine as to success.

From this time I lost sight of the patient altogether, until the 18th of May, 1875, when I received a letter from him, from Paris, which runs thus:—" You must, by this time, have almost forgotten me, but when I recal to your mind that it was you who saved my life, at Mercer's Hospital, I think it was in the year 1858, by excision of the elbow-joint. Since then I lived in London for some years; but for the last seven years I have been in this fair city. You made such a good cure of me that, during the dreadful Siege of Paris, sooner than act the coward and run away, I joined the Foreign Legion and took up arms in defence of the city. I took part in the two days' fighting at the Plateau d'Avron, and several other minor engagements; remained here during the terrible Commune, and, thank God, escaped all. I am, and always have been, in the enjoyment of excellent health, with perfect use of my arm. I often thought of writing to you, but through all the terrible excitement of the last four years, one was not sure of themselves. I hope this may find you well. I have never forgotten you.— M. REYNOLDS."

Plate VII., traced from a photograph by Courtmioux, Paris, which he enclosed in his letter, shows his condition now.

No doubt youth or adult life are essential elements towards success in cases of resection, as youth is likewise towards repair in many diseased joints in childhood. Years of experience have confirmed the precepts which I laid down so many years ago, when reporting this case, and writing on the subject. They seem such stern truths that I must insert them here:—" *I would wish to say one word about the management of those cases of diseased joints in infancy and childhood. It is impressed strongly on my mind— nay, more, I would say it is my conviction, from a long and careful study of such affections—that those severer operative measures will very seldom, indeed, be either warranted or called for.* This rule applies even with greater force to the upper extremity. I could cite numerous instances, most unpromising at first sight, where, by judicious surgical interference, the limb has been rescued. *Childhood does not seem to be the recipient of the same wasting, destructive fever that takes possession of the constitution in the adult; and, during the periods of early life, repair is quickly brought about, owing to the extreme vitality of the living tissues, the part destroyed is, with amazing rapidity, regenerated and restored.* How often do we

PLATE VII.

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY,
ON EXCISION OF THE ELBOW JOINT.

see this fact forced upon our observation? A child is brought into hospital, having been thrown down by a car rapidly passing by; the wheel has lifted a large flap from the skull; the violence which has torn and lacerated the soft parts has denuded the bone, probably chafed away its superficial surface; and yet, though this injury would surely kill the osseous tissue in the adult, in a few days we see the injured parts pink, reddish, covered with granulations sprouting from every point and incorporating themselves with the flaps, equally prepared by the hasty removal of all gritty matter and contused tissues. *So is repair in the young to be looked forward to, to be depended upon as a certainty; and, therefore, again I repeat, no hasty removal of diseased joints in childhood.*"— (Butcher, Operative Surgery, p. 202).

CASE II.—*Enormous Fibro-Cellular Tumour extending over left side of the Neck and Face—Successfully Removed by Operation, and with little deformity.*—Julia Sullivan, admitted to Sir Patrick Dun's Hospital, October 10th, 1876. The history of this remarkable case is as follows:—When a young girl, seventeen years of age, a small tumour appeared beneath the angle of the left side of the inferior maxillary bone, and which gradually and steadily increased up to the present period, the patient being now forty-nine years of age. On admission to hospital, the magnitude of the tumour was very great, measuring, in its longest axis, eighteen inches; and, in its transverse middle, a line passed over it measured sixteen inches. The entire face was distorted by the growth, and thrust over towards the right side; the left eye-lid was shoved over, as well as the mouth and nose; the whole head was turned towards the right side, owing to two causes—the blocking up of the entire space from the left side of the head to the clavicle; and the effort exerted by the patient to counterbalance the dragging weight of the tumour towards the left side. The patient's aspect was truly miserable; the anxious piercing look for relief was very pitiable. The persistent growth of the tumour from her early life until now, though borne well, and, with its many annoyances, culminated in a determined will to seek relief. More than a year ago this creature made an application, through a gentleman, to me for admission to hospital and for operative relief. This application was acceded to, and a letter for admission forwarded; after a short time I heard that she was terrified at the prospect of the operation and declined my advice. More than a year had passed by, and now the same kind person writes that she, the young woman, solicits admission to the hospital and is willing to undergo any operation that I might think right to perform. This application I at once acceded to, and so the patient was admitted on the above date. The enormous proportions of the tumour were as already

mentioned. And the beautiful pictures (Plates VIII. and IX.) show most accurately the characters and position of the tumour, both in front and back view. Nothing could be more beautiful and artistic than the photographs from which these plates were copied, and which were executed by Messrs. Robinson and Son. They have been committed to the stone with the greatest accuracy by Mr. M'Connell, the distinguished artist, at Mr. Forster's great printing establishment in this city. I have also a splendid cast of the patient, taken by myself and placed in my museum. The aspect of the growth from its formidable dimensions and deep attachments, were very discouraging. On carefully handling the tumour, considerable motion was prevalent throughout, but its bulk was so great that it pendulated in every direction in which it was moved, when pressed upwards or downwards beyond a certain point, the tensive bands were raised out, showing some constriction in these localities, and when undulated from side to side it was equally restricted and firmly tied; the whole feeling of the tumour was elastic, with hardened nodules throughout, and firm fibrous bands intersecting its proportions; fatty matter was deposited in abundance, while the fibro-cellular structure, in great quantity, condensed the entire mass. The integuments were thinned and strained over the entire mass, and numerous small veins traversed the surface. There was no pain in the tumour from first to last; there was now no pain, on or before handling it for a considerable time, seeking for its deep connexions. On lifting up the entire growth from the side of the neck and face, deep-seated roots and connexions could be, as anticipated, clearly felt, at once showing the difficulties that surrounded its removal; and here I would impress how deceptive the floating undulating character of the tumour when handled on the surface, as contrasted with the true characteristics of its deep relations when carefully searched for. The tumour had no characteristics of malignancy about it—its long life and history was against this; the absence of all pain, the healthy appearance of the girl, free from depraved cachexia, the quiet pulse, altogether stamped the growth as not malignant. Then the more prolonged manipulation of the tumour proved it to be a harmless one in its consistence and integrity, though so formidable from its bulk and implication with all the important parts deep in the neck. The history of the operation, the dissecting out of the mass, will elucidate its deep connexions.

On the 14th of October, 1876, the patient was placed on the operating table, and ether was administered, producing satisfactorily its full anæsthetic effect; and here I may remark that the pulse, which had been very shabby (though brandy and water was given half an hour before), rose very perceptibly, with more steadiness and volume. The patient being fully narcotised, the operation was commenced. Standing on the left side of the patient, I made two elliptical incisions through the skin, beginning at the lowest part of the projecting mass, and continued

PLATE VIII.

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.
ENORMOUS FIBRO CELLULAR TUMOR—FRONT VIEW OF PATIENT BEFORE OPERATION.

PLATE IX.

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.

ENORMOUS FIBRO CELLULAR TUMOR—BACK VIEW OF PATIENT BEFORE OPERATI

upwards for about ten inches, and then dissected back the flaps throughout this extent, upwards and backwards, constantly using the handle of the knife and my index finger to detach the covering from the growth; this being done on either side as far as practicable, the incisions were continued upwards to where they met beneath the ear. A similar process was executed here, the skin on either side was dissected with the same caution up from the tumour, the handle of the knife frequently being had recourse to; and when sharp tight bands prevented to further detachment of the covering, the sharp blade was had recourse to most efficiently. And so now the whole superficial surface and sides of the tumour were perfectly cleared, two large vessels had to be ligatured. The dissection having been so far accomplished, it was now apparent how deeply entangled lay the base of this morbid mass. With the greatest caution the dissection was proceeded with; layer after layer of fascia being divided; some teased out before division, and in the line of dangerous parts, others stretched up, and against the light, to show transparency before division; and so, with the same cautious steps, the operation was conducted; so deep, indeed, was it carried, that the growth was turned out of the digastric space—the digastric muscle being clearly brought into view—the sheath of the carotid artery with its contents was clearly revealed throughout the entire extent of the neck; three processes of the tumour passed very deep; one thick process fully four inches long and one wide, forced itself between the internal and external carotid arteries, just above the bifurcation, and the force necessary to detach and draw out this root constricted it, in a marked way, on its delivery; so likewise in the other two roots—distorted and compressed from being drawn out of their constricting chambers, one deep from the digastric space, the other lower down and equally deep. So after a long and tedious dissection amongst parts most essential to life, and with which this growth was in the closest proximity, the entire was safely removed; three large arteries sprung, towards the close of this formidable operation, and were instantly ligatured. This case revealed a remarkable sight—to see in the living subject, the entire side of the face and neck made a perfect dissection of—the carotids, the carotid sheath, and common vessel; the digastric space, and the bounding lines of it, all displayed; the lingual nerve, submaxillary gland, and other important parts revealed in front of the neck; while at the angle of the jaw, the masseter muscle on the external side, and the internal pterygoid on the inner, were brought into view, as well as the deep parts of the parotid gland, the facial and internal maxillary arteries, each taking their respective places; the occipital artery, in its early stage, was also clearly seen. During this extensive dissection very little blood was lost. The entire surface being dried, was then brushed over with a strong solution of chloride of zinc and water, twenty-five grains to the ounce. I invariably use it, as I firmly believe it everts

unhealthy inflammation, it corrects fœtor, it modifies suppurative action, and it conduces, by its watery evacuations from the cut surfaces, to lessen tension, and to reduce temperature. The edges of these extensive flaps were then approximated and brought together by several points of the wire suture; the flaps, particularly the posterior one, was well supported forwards with compresses and adhesive straps, the cheek being drawn fully forward, so as to leave sufficient material, that it might remain full and prominent as the sound one. The patient quickly recovered from the ether, with a vigorous and steady pulse, and was then conveyed to bed; in half an hour after a full opiate was given. Ordered beef-tea and wine every third hour, and a grain of opium every third hour.

Visited at 4 o'clock; has had some sleep; free from pain, and has taken beef-tea and wine and water; pulse 108, steady.

October 15th.—Slept well; took tea and toast; no pain; kidneys acting well; pulse 106; a large quantity of watery sanguineous fluid escaping from the wound. Beef-tea, wine, and full opiate every sixth hour.

16th.—Slept well; no pain this day. Removed all dressings, by cutting them across, without pain, patient never complaining. Gentle pressure over the surface brought out a large quantity of sero-sanguineous fluid. Re-dressed as before. Beef-tea, six ounces of wine, and a grain of opium every sixth hour.

17th.—Slept well; pulse 102; heat of body considerably reduced; skin moist; kidneys acting well; the whole expression of face improved, being bright, and intelligent, and happy; sero-sanguineous fluid flowing freely, and so best—this I have invariably found to occur after the chloride of zinc application, and for reasons mentioned already I think highly satisfactory; no pain on the removal of the straps, &c.; readjusted all as before.

18th.—Slept well; pulse 98, soft; no headache or pain complained of; took breakfast of toast soaked in tea, with appetite; thirst gone; kidneys acting freely; swelling considerably reduced about the cheek and wound; watery fluid still flowing, and can be easily pressed out; however, it, too, is much diminished in quantity. Dressings reapplied as before. Wine, broth, milk, and opiates, to be steadily continued. 4 o'clock, most comfortable; bowels opened freely since morning.

19th.—Slept all night; pulse regular, 96, and steady; took all her nourishment with appetite, and her breakfast, toast and tea, this morning; kidneys acting freely, and bowels opened; skin cool; tongue clean. The constitutional symptoms all most satisfactory. The dressings being removed, the wound looks very well, the swelling in flaps and surrounding parts very much diminished; still a watery fluid, on pressure being applied, escapes freely through several parts of the extensive wound; but sanguineous discoloration nearly all gone. Reapplied straps, pads,

and bandages, as before. Nutriment freely given—broth, milk, wine, every second or third hour. The full opiate night and morning.

20th.—Steadily improving; slept well; pulse 80; skin moist; kidneys acting, and bowels moved twice without artificial means; wound looks very well, and at swollen edges contracted. Dressings applied as before, with the injection of chloride of zinc solution beneath the parts. Food, wine, and opiates to be continued.

21st.—The patient has had an excellent night's sleep, and expresses herself as more comfortable than ever since the operation was performed. Pulse 90, soft, and very compressible; bowels freed; kidneys acting well; skin moist. The whole expression of the patient's face is greatly improved, being one of quiet and repose. The swelling all around the wounded parts is considerably diminished, and pressure is borne over the entire surfaces with much less wincing. Again, the fluid passed out is greatly improved, being changed from the ichorous fœtid discharge to a fluid more characterising pus, and not nearly so offensive as before. The parts all lay more evenly and steadily together, and scarcely any inconvenience was complained of during the dressing and readjustment of straps, pads, and bandages. To continue diet, broths, wine, and full opiates night and morning.

22nd.—The patient slept quietly throughout the night, and this morning feels greatly refreshed and quite happy, the miserable forebodings which haunted her, that she could not recover, have now nearly disappeared. Pulse 90, soft and compressible. She has taken her food in quantity; and, on the whole, she is much stronger. The wound looks, however, pretty well; the swelling in all directions is greatly gone down, particularly in front of the jaw, and also behind the ear; all tenderness about the windpipe and in front of the neck nearly dispersed; the discharge, on pressure being applied by a sponge in each hand, is lessened greatly in quantity, and altered materially in character; it is now of a purulent nature, thick and closely appertaining to the consistence and colour of healthy pus, the fœtor is greatly diminished; all the matter being steadily pressed out, lint soaked in a strong solution of carbolic acid was pressed along the entire extent of the wound, and three layers of lint, so soaked, laid on, the lint soaked in carbolic oil laid over all, and straps of plaster applied so as to retain the entire in position, with gentle steady pressure upon the pads, all being supported by a few turns of a bandage. The patient was then raised considerably, nearly to a sitting posture, and so supported with pillows. Ordered beef-tea every third or fourth hour, and eight ounces of wine; opium to be continued night and morning. There is a very striking improvement in the aspect of the patient to-day; much of gloom and sorrow had passed away, and she looks more confidently forward towards recovery and "going home." Now these feelings engendered towards recovery, and developed by

hope, are, in my mind, and from extended observation, of much consequence; the patient believes in exemption from disease—because the operation has been successfully performed, and willingly applies the mind to follow assiduously out all the medical directions that may be imposed.

23rd.—Has slept well throughout the entire night. Pulse 88, soft and compressible; temperature natural. The wound looks more favourable in every respect; there is no unnatural inflammation about it, nor increased discharge, but that which flows is of a far more healthy character. All the deformity of puckered flaps greatly diminished by gentle steady pressure. Ordered abundance of beef-tea, wine six ounces, eggs, and opium. Dressings applied as before, and with great confidence; compresses soaked in carbolic acid solution, to correct foetor, immediately over the irregularities, &c., of the entire tract of the wound.

24th.—The patient has slept well, taken all her nourishment and stimulants, also her breakfast of tea and toast with appetite. Her pulse has come down to 86, soft, full, and healthy volume; her wish for food and stimulants greatly augmented. The dressings being removed, the entire cheek and neck seemed marvellously reduced, the swelling on all sides greatly diminished; on pressure, gently applied by sponges wrung out of hot-water with both hands, a considerable quantity of healthy pus pressed out, though not nearly so great as on the last few days; several shreds of deadened cellular tissue and fascia likewise pressed out; the diminution of the swelling, and the lessened state of the swollen and thickened parts around is most striking and remarkable. Dressed the wound as before; lint in shreds being soaked in carbolic acid lotion, and gently pressed into all the irregularities and separations in the margins of cut surfaces, compresses were laid over as most suitable to support and sustain the long flaps, by pads and adhesive straps carefully laid on, all steadily held in position by a bandage evenly and regularly applied beneath the chin and over the head, until all the parts maintained in position by the adhesive straps and pads were secured so that they could not slip. To continue the beef-tea every third hour, and the wine six ounces, and also the opium morning and evening; milk and egg beaten up with brandy twice a day.

25th.—Slept well; pulse 86, soft and compressible; bowels acted on; temperature natural; skin moist; has taken all her food with appetite. On removing the dressings the parts lie admirably in position, the foetor nearly gone, and the discharge thickened in consistence and far more healthy in appearance; all the ligatures, five in number, came away on the gentlest traction. The dressings applied as before; shreds of lint soaked in a weak solution of chloride of zinc, passed into all the irregularities and crevices, lint soaked in carbolic acid laid over, and adhesive straps applied over the compresses steadily from without inwards with

a gentle pressure, so as to direct all secretions towards the wound, all maintained in position by a few turns of a bandage acting with the same intent. To continue opium, wine, beef-tea, and eggs as before, in liberal quantities.

26th.—Slept quietly all night; the opium and stimulants steadily given in full quantities; pulse 96; all secretions healthy. On dressing wound, fœtor all gone; discharge considerably diminished; pads, adhesive straps, and bandage with compresses as before. To continue opium, wine, &c., as before.

27th.—Has slept quietly during the entire night; no pain or uneasiness in the neck; pulse 84, quiet, soft, and compressible; kidneys acting healthily; bowels opened naturally; takes her food with appetite. The wound looks very healthy; swelling on all sides considerably reduced, also the discharge lessened in a most conspicuous manner; fœtor all gone; and the fluid now thick and bland, almost "laudable pus." Cut out all the wire stitches, and it is most remarkable to observe to what a steady extent union of the edges of the wound have been effected, and it is surprising that no thinned part of the extensive flaps sloughed or died from want of vitality, or that no large sloughs of deeper parts were cast off— ·no, the discharge is healthy, quite free from sloughs or shreds of deadened cellular tissue; approximated the edges with smaller and more effective straps of plaster, securing, by smaller pads upon elevated portions, more direct and effective pressure; larger compresses then, as before, applied over all, and retained by long straps and bandages as before. To continue wine, opium, beef-tea, and pounded meat.

28th.—Slept all night; no pain whatever; pulse down now to 80, soft and compressible; secretion of kidneys quite natural; skin moist; takes her food with appetite. The wound looks most healthy in every way; scarcely any discharge, and that secreted of the most healthy kind. readjusted dressings as before. To continue wine, opium, beef-tea, eggs, &c., as before.

29th.—Improved in every way; more cheerful; pulse 76, natural in beat; no pain whatever; slept quietly throughout the entire night. The wound looks nearly healed, and the thickened irregularities of the flaps all considerably reduced and flattened; pads and bandage readjusted as before. The whole appearance and countenance of the woman is changed for the better in a most remarkable degree; she looks bright, good-looking, and happy.

30th.—Patient so well, so free from pain, and wound so nearly altogether healed, allowed to get up for a couple of hours. Pressure by pads, straps, and bandages applied as assiduously as before, so as to mould all the parts into form. Diet most liberal and eight ounces of wine; full opiate at night.

31st.—Very little discharge from the wound; swelling and thickening

of the parts all subsiding under the carefully applied pressure, which was applied most sedulously as before. Appetite very good; sleep uninterrupted; pulse steady at 72; temperature natural. Allowed the patient to get up this day for six hours.

November 1st.—The patient slept quietly and better than for many nights, owing to being out of bed and moving about the previous day; her whole aspect of despondency has vastly improved, and now that she is up and walking about the ward she is very happy and looks with a certainty of being well and going home. The wound discharges very little; no pain whatever; pressure applied steadily to reduce thickening of the flaps, &c.

2nd.—The patient has slept quietly, and feels much benefited and refreshed by being up the previous day. The terrible depression and shock which she sustained, rendering her almost out of her mind (indeed, at some periods she was perfectly maniacal), is steadily passing away, and she is beginning to feel a confidence—as I stated on yesterday—that she will recover, and looks steadily towards perfect recovery and going home. The wound is nearly healed, and it is wonderful the small amount of deformity that remains; pressure by pads, straps of plaster, and bandage reapplied so as to flatten all projecting parts, and to press towards the track of incision any matter secreted within. General health greatly improved. To continue opium at night, ten ounces of wine, beef-tea and bread several times in the day.

3rd.—Slept the entire night, and has taken all food with great relish. The wound scarcely discharging at all. Brushed over the interior of the flaps with a fifteen-grain solution of nitrate of silver; pads and straps accurately adjusted, so as to make efficient pressure. To continue opium at night, and nutriment freely; both stimulants and nutritive diet as before.

4th.—Steadily improving; pulse quite natural; temperature normal; sleep quite undisturbed throughout the entire night. The wound most healthy; discharge only natural for the unhealed surfaces, which now are very limited; the thickening of all surrounding parts considerably lessened by the gentle pressure judiciously employed. To continue the bark mixture as directed, and also the opium at night, and the full supply of nutriment and stimulants.

Since last report the patient has steadily improved, both locally and constitutionally. The irregularities of the integuments—over the surface from which the enormous mass was removed—have considerably contracted, and the folds, by the gentle pressure made, have been greatly diminished; there is but little discharge now from these extensive surfaces externally; all the flaps being gently laid down and held in position, have been altogether agglutinated to the parts beneath. The little amount of discharge is quite healthy, free from odour. The entire surface and united edges

PLATE X.

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.

FRONT VIEW OF PATIENT REPRESENTED IN PLATE VIII. AFTER SUCCESSFUL OPERA

PLATE XI.

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.

BACK VIEW OF PATIENT REPRESENTED IN PLATE IX. AFTER SUCCESSFUL OPERATION.

freely brushed over with a fifteen-grain solution of nitrate of silver for the last few days, and with the most satisfactory results.

9th.—The improvement in the patient's entire aspect is most remarkable; she is bright and happy at last. On looking at the patient's front face no one could conjecture, for a moment, that any operation had been performed upon her—the gentle, quiet look, which evidently now had been the natural expression, is quite restored. There is but very little discharge from the wound; all the flaps and subjacent surfaces being long since grown together—there is but one small point in the centre of the extensive incisions, from which comes a little healthy pus. Dressed as before—viz., caustic solution applied to the surface, a few folds of lint soaked in carbolic lotion, compresses over the projecting parts, and over all simple dressing retained by a few straps of adhesive plaster. I left the head free from bandages, to allow the matted hair to be separated and combed out. Effervescing bark mixture, wine, and full opiate at night, to be continued.

11th.—The only unhealed portion of the incision, noticed in the last report, is now nearly all healed; the puffed-up condition of the parts around is nearly, and in some parts altogether, subdued by the gentle, yet uniform pressure applied from day to day. There is no discharge; and, in every way, the constitutional condition of the patient is, in a most marked manner, improved. Diet of the most nutritious kind and six ounces of wine freely given. The quinine mixture continued. The patient, as before mentioned, is up every day and walking about.

13th.—There is now no discharge from the wound; a short line in the centre of the incision just skinned; there is no tenderness anywhere about the extensive region from which the massive tumour was taken away, even on pressure used in the most searching manner. The small surfaces remaining open were freely brushed over with the nitrate of silver solution, a few thin slips of lint soaked in a lotion which I am very fond of using when the last healing efforts are delayed—namely, one drachm of tannic acid and half a drachm of sulphate of zinc to six ounces of water—were laid on all, being supported, as before, with compresses and adhesive straps and bandage.

28th.—The entire healed; all the elevations levelled; scarcely any deformity, none seen when looking at the patient in front, and the marks behind the jaw are all easily covered by a ribbon tied over the head. The whole appearance of the woman, as contrasted with her deformity and sad condition when admitted to hospital, is most remarkable and highly satisfactory. I have a fine cast taken from her now, which affords a most remarkable contrast to the one which I took on the patient's admission to hospital; and the beautiful pictures (see Plates X. and XI.) traced from the admirable photographs by Mr. Robinson, taken from the woman the day before she left the hospital cured, and for home (December,

8rd, 1876), contrast in a most remarkable manner with her condition when admitted to hospital, as represented in Plates VIII. and IX., and afford striking examples of successful surgery.

The tumour, after its removal, being examined, showed all the characteristics of its diagnostic synonymes. When freed from the constringing fascia, many lobules and irregularities appeared upon its surface; many projecting for more than an inch, and fully the size of a hen's egg; deep-seated trabeculæ binding the structures within, while the external moderating force of the fascia before the division restrained backward the growth, making the external surfaces comparatively smooth, though projected in certain localities and assuming a slightly convex form. The pressure of the external fascia must have been considerable, as determining and modelling the outer form of the growth. On close examination with a lens, fine arterial ramiform injection pervaded minutely the intervening tissues of those lobules, and, no doubt, was highly accessory to its steady and determined growth. The long processes of the tumour, already referred to, that passed deep into the intricate spaces described, were highly vascular throughout, and steadily contributed new material towards a persistence in deposit where the least appreciable resistance was met with, and so the deepest roots of this morbid mass selected the easiest course for its increase. Thus, then, the appearance represented by the tumour confirmed the nature of the growth and its proper nomenclature, as fibro-cellular, with large fatty depositions of a granular structure, made firm by unusually thickened trabeculæ, with cellular and elastic bands. This arrangement was quite patent by the aid of the ordinary lens, and so clear that I did not consider a microscopic picture necessary to elucidate its nature further, all the characteristic features of the case being so apparent. There was not the least portion of the diseased structure left behind. The capsule was perfect throughout, a part of it behind being calcified to the extent of an inch and a half. A fine cast of it was taken, which shows all the integrity of the tumour, in some places ruffled on the surface from torn up adhesions; and the prolonged roots also show their rounded covered ends, beyond which nothing had passed or sprung. And so the perfect and entire removal of the tumour, as clearly demonstrated.

CASE III.—*Double Complicated Hare-lip, with entire absence of the Palate, and with Projecting Intermaxillary Bones straight forward, occasioning the most hideous Deformity both in Mother and Child, each most Successfully Cured by Operation, and with scarcely a trace of Deformity.*—Christina Gallagher, aged twenty-seven years, married three years and a half; she was born with a double complicated hare-lip. When two years old the centre-projecting piece was cut away and no further operation had recourse to, the gap between the maxillary bones being excessive—fully

PLATE XII.

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.
DOUBLE COMPLICATED HARE-LIP IN MOTHER AND CHILD,
FRONT VIEWS BEFORE OPERATION.

two inches—and the lips adherent to each, leaving the most hideous deformity. When twenty-two and a half she got married, and, in eleven months, had a male child, which was born with a cleft palate, and a single fissure in the lip on the left side and into the nostril, with projecting intermaxillary bones from the right side; the child only lived three weeks, and then died of convulsions. In somewhat more than a year after she had another male child, the little fellow which I now represent, with double cleft palate, double fissure in the lip, projecting intermaxillary bones straight out, representing the most frightful deformity. The husband and every member of his family were healthy and well formed, and the mother, though deformed herself, had several brothers and sisters without the least mark, and her father and mother were well formed, and handsome peasants.

The child, James Gallagher, aged two years, was brought up from the country, and admitted to Sir Patrick Dun's Hospital with his mother, October 23rd, 1876—a more serious or imposing case could scarcely have been contemplated. I have figured in my work on Operative Surgery many cases of extreme deformity, depicting double complicated hare-lip, and deficient palatal plates at either side of the septum, with projecting intermaxillary bones and massive septum, also with entire deficiency even of the septum in the two posterior thirds. Yet none of those cases exceed the horrible deformity portrayed in the subject of the present remarks— the wide fissures between the maxillary bones on either side and the projecting ossa incisiva, united together and projecting from the vomer straight forward and very massive, I have seldom seen equalled. The prominent projecting portion was most unusually massive, being fully three-quarters of an inch thick, with a very thick root. The vomer itself was moderately thickened, and all its anterior structure hardened like ivory. This solid piece of bone stood out almost vertically, and upon it rested a small triangular piece of the lip, united in the centre, corresponding in size, and in development, and union in the mesial line with the incisive projecting united bones beneath, and those united soft parts were matted to the support beneath. This entire central portion, bone and flesh, stood, as already stated, in front, occasioning the most hideous deformity. The nose was shockingly depressed, being spread out quite flat, the alæ being connected and expanded upon the ill-developed maxillary bones. The apex of the nose was hooked down tensely, owing to the wide gap between the maxillary bones and the necessity for being attached to and meeting them.

The appearance and entire character of the deformity are most truthfully represented in Plate XII., a beautiful photograph by Mr. Robinson, accurately traced by Mr. M'Connell, of Mr. Forster's lithographic establishment; and I know no instance in which the mother and child are represented with this same horrible complicated hare-lip in the

same picture. The front view is most truthful, as well as also the pro-
files of mother and child in Plate XIII. On October 28th, 1876, I
operated on the child after the following manner :—The little fellow was
rolled in a sheet, mummywise, the arms being carefully restrained,
and so was placed in the arms of an assistant seated on a high chair, the
head being placed well back, and resting on his left shoulder ; the most
important assistant stood behind steadying the head. Standing in front
of the patient, the central soft portion over the intermaxillary bones
was seized with a tenaculum from below upwards and freed most ex-
tensively from the projecting ossa incisiva beneath far back for fully a
quarter of an inch behind the front, where the regular septum should
have sprung from. This being carefully completed, the projecting bony
mass was dealt with after this fashion—the edges of the mass were cut
off with scissors towards a V shape ; it was then clear that the mass, when
clipped accross by my vertical forceps through its conjunction with the
vomer far behind, was too long when forced back by the pincers covered
with leather, so that I at once removed the thickened mass off the pro-
jecting portion behind, thinning it so that it was quite manageable and
readily pressed back. The stalk being divided, as already described, and
the mechanical division given, the soft parts in either side of the bone
were saved, and so the life of the preserved central portion insured and
sustained. On steady examination it was found that all was done for the
saving of this central piece so pared away as to admit its being forced
back to fill up the space between the superior maxillary bones, and to
afford a steady support to the lateral portions of the lip when approxi-
mated, and, above all, to give a foundation to the V-shaped piece, cut out
to make the septum. These steps of the operation being rapidly carried
out, the lip on the right side was seized with a tenaculum, it being
entered just at the termination of the curve in the lip ; it being so drawn
down gently on the stretch, it was lifted up and freed extensively from
the maxillary bone, and so was the right ala of the nose, and the entire
most extensively dissected and freed outwards, until the lip and nose
could be easily lifted inwards. A similar mode of proceeding was carried
out on the left side ; all the parts, lip and nose, being so extensively freed
from the bones, on being drawn forwards, confirmed the opinion that
enough had been done in this direction. Next, the border of the lip,
from the point where the tenaculum on the right side had entered, was
drawn open and made tense, and my curved scissors was applied external
to, and close up to, the tenaculum, its curve being brought to bear a little
above the red border and lower part of the lip, so as to make this part of
the incision concave, while the upper part of the incision, as well as that
into the nostril were quite straight. The left side of the lip was seized
in the same way, and dealt with after a similar manner, both edges being
most accurately cut. I inserted a fine long thin needle external to the

PLATE XIII.

Drawn from a Photograph.   Forster & Co., Lith., Dublin

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.
DOUBLE COMPLICATED HARE-LIP IN MOTHER AND CHILD, SIDE VIEWS BEFORE OPERATION.

ala nasi on the left side, fully three-quarters of an inch, passed it across through nearly the apex of the central preserved portion, and through the lip on the opposite side. The transfixion throughout was perfect, straight, and decided. I next passed a needle just above the border of the red margin of the lip, and from left to right, entering it fully three-quarters of an inch away from the cut surface. A ligature of soft, not very fine, silk was cast in a figure of ∞ form first around the upper needle, making gentle steady tractive pressure on the included parts. The parts were well pressed towards each other before the silk was cast around the needle, for though compressing and holding the parts together, yet they were gently forced forwards upon the needles to approximation, and then prevented from retracting by the cord thrown around the needle without any biting constriction. A third needle was passed from left to right, regulated by the same caution, between the two; it was entered fully three-quarters of an inch beyond the margin on the left side, exactly on a line with those already inserted; it too was made to traverse steadily its way through all the tissues close to the mucous membrane. The ligatures were pressed steadily and carefully above those several needles in the figure of ∞ form, each ligature being quite independent of the others. I lay stress upon this point, as any crossing of the ligatures from one needle to another invariably produces a kind of unnatural oblique constriction, which makes the part resent it, most likely by ulcerating out the needle, and letting it go free—no, the pressure should let each needle and suture cast around it bear its own burden. Besides, if the ligatures are cast across from one needle to the other, it tends to drag up and shorten the cicatrix, which would be most reprehensible, as such a disposition lurks in the newly-created line. Again, such a proceeding would seriously interfere with the withdrawal of a needle if required, a sequence which, under certain conditions, may be required, leading to the insertion of another in a more healthy part. The ends of the needles were then clipped off, and next there was applied a wide strap of adhesive plaster, about six or seven inches long, fully two inches and a half wide, curved out on one margin so as to lie in front; this was applied to the temple on one side, and then brought beneath the chin to a corresponding part on the other side, the cheeks being well supported forwards—thus when the plaster was in position the cheeks could not again recede. I prefer this method to Hanesby's apparatus, which, if put on with sufficient force to keep the cheeks forwards, creates, in a short time, so much swelling as to require its removal—its abandonment altogether. Now, the operation being finished, nothing could be more satisfactory than the way in which every part lay. Shortly after the operation the little fellow took some warm milk. I likewise, as I always do in those cases, ordered small doses of tincture of opium so as to narcotise the child for some hours. The child went on very quietly, and

took quantities of milk during the three following days. All this time the opium was guardedly persisted in, quieting the child most effectively, and, on the fourth morning after the operation—ninety-six hours—I removed the needles. The child being carefully steadied and the cheeks held forwards, I gripped the upper needle in a forceps and steadily withdrew it from right to left, making pressure with the point of the index finger of the left hand, and maintaining the lip in the contrary direction with a like amount of force, so that any tendency to separation was guarded against. The lower needle was next removed, and so likewise the central one, after a similar manner. The adherent threads were left undisturbed in their position, and a few cross straps carefully put over them. While drawing the cheeks well forwards, a fresh piece from temple to temple and beneath the chin was also put on, with the desired effect of preventing the cheeks receding.

November 1st.—The child being steadied, and the cheeks well pressed forward, all the straps were taken off, and the twisted sutures came away also, revealing the condition of the entire lip beautifully healed throughout, the central piece being most perfectly united to the cut surfaces of the flaps on either side, and the even-united line of the red border of the lip is most perfect, there not being the least trace of notch below. This, as I have stated before, results from the curvilinear incisions secured by my curved scissors. The projecting maxillary bones in front preserved this admirably behind the united soft parts, affording an admirable support to them, and of great advantage in restoring the natural contour of all the parts about, and in a special manner as yielding a fixed basis for fixing the spread-out alæ nasi, and securing in position the V-shaped septum. The cheeks being well held forward, the straps of plaster were adjusted as before, and a long narrow strap passed in a convex way over the nose so as to depress it a little, and so give steadiness to the newly-made septum. The under chin-strap also put on as before. The opium still continued. The dressings were applied every day with the same care, and, on the 12th, they were removed altogether, the case being perfectly well and scarcely any deformity at all. See Plate XIV. for the front view of the little child, and Plate XV. for the side face, and, on contrasting those with Plates XII. and XIII. before the operation, it must, I think, be admitted that every benefit was secured for the little creature by the troublesome operation to which he had been submitted.

Operation on the Mother of the Child.—Now that the operation on the child was so successful, on Saturday, 18th November, 1876, I determined operating on the mother. I deferred the operation on her in order that she might watch over and care the infant throughout his recovery. Though doubtful at first as to her submitting to any operation, yet now

when she saw the child restored with scarcely any trace of deformity or marks of the operation, all her wavering yielded, and she proposed to me when I thought I would operate upon her. Her condition, as represented in Plates XII. and XIII., copied accurately from the splendid photograph by Mr. Robinson, could not be exceeded in misery; nothing could be more repulsive than her countenance; nothing could be more unpromising as to a satisfactory operation—at least one that could at all improve the frightful appearance of the patient. The unfortunate interference with the deformed mouth when the woman was a child, and the unmeaning interference with the central depressed part, tended greatly to the fearful aspect of the patient; and her deformity, now grown into adult life—the wide-spread gaping dark space from stunted maxillary bones from one side to the other; the entire absence of the clipped-away intermaxillary bones, insufficiently done—revealed the entire apartments of the nose and the capacity of the pharynx beneath, besides the wide-spread alæ nasi bound down far away on left and right sides to the diminished imperfectly united superior maxillary bones, contributed to add to the shocking deformity. There was a projecting strap spine of the vomer, from which the central and most important part was clipped away, standing prominently forward, with the thin abridged skin piece over it, all most unsatisfactorily arranged for the due performance of what, under the present condition, might be looked forward to as yielding hopes of a satisfactory operation.

On the 18th of November I operated on her in the following way :—I did not use any anæsthetic; first, because I knew I could perform the operation rapidly; and, secondly, I wished to have her in the sitting posture. Being seated on a high chair, a stout piece of wood was run through the arms of it and across her thighs, and so guarding against any violent effort to jump up. During the steps of the operation a sheet was wrapped round the woman, just as in the child's case, binding down her arms and so preventing any upward movement with them. The head of the woman was steadied against the left shoulder of an assistant standing behind the chair. I at once proceeded to detach the small piece of skin matted down upon the anterior massive piece of the vomer, from which the ossa incisiva had been cut away. The piece, pierced by a tenaculum at its angle and put upon the stretch, was dissected backwards and upwards as far as possible, leaving quite naked the piece of bone just described. The tenaculum was taken hold of by an assistant and lifted up when I proceeded to deal with the bone. The stalk from which it sprung was cut far back by my vertical cutting forceps, invented for this purpose, while the mucous membrane and some denser tissue at either side of it were left uninjured, and so insuring a full vascular supply for the support of any portion of the bone I might consider right to preserve in front. The piece was so thick that when

turned back it projected too much in the centre, and, if allowed to remain so, would have made too great a pressure from behind upon the soft parts when the ligature was cast across the needle, and so cause ulceration and non-union; therefore, with a flat forceps I cut off the convex projecting portion, leaving a thin even piece behind, composed of a thin plate of bone, and sufficiently covered in the soft parts, and quite adequate, at least, to giving a steady groundwork upon which to rest the most important part, the centre piece; and secondarily, to steady the divided lateral portions of the lip when lifted up for union with it. The central bony part being so disposed of, the detached soft part over it was cut in a triangular form with the straight scissors, sufficient tension being kept up by the tenaculum inserted before, and now rendered accessory, by a little traction, to the even division of the parts in the best way for the formation of the septum. A tenaculum was next inserted into the left portion of the lip, just at the commencement of the curve, when by it the lip was drawn well upwards and outwards so as to facilitate the free separation of it, together with the ala nasi of that side. The parts were most extensively cut and lifted up, fully as far as the infra-orbital foramen, the edge of the knife being kept very close to the bone so as not to injure the infra-orbital nerve. The right portion, together with the ala, were dealt with exactly in a similar manner, being freed as extensively from the bone, and with the same guarded use of the knife. The parts were then gently drawn forward to make sure that they were sufficiently freed to come together without strain; tension being then put upon the left portion by drawing with my left hand the tenaculum downwards and outwards. Pressure being put upon the facial artery by an assistant, I applied the curved scissors just external to the tenaculum, the lip well in between the blades, so that the chief curve of the scissors rested a little above the red border, while the straighter portion lay upon the lip straight with the nostril, then the part was clipped off by one stroke of the instrument. A precisely similar portion was cut off, after the same manner, from the right lip, the right hand with the scissors being, of course, beneath the left one holding the tenaculum. A long fine needle was passed from left to right, three-quarters of an inch outside the ala, taking in its transit the ala itself, and, through it, the solid portion of the central piece, and so bringing up the soft parts in contact. The needle was forced through to a corresponding point in the right side, a long silk ligature was cast around the needle in the figure of ∞ form, and the parts steadily pressed forward before the cord was strained or tightened, the cut surfaces of the ala and upper portions of the lip, on either side, lay steadily up to the central V-shaped portion forming the septum of the nose. Behind the soft solid part retained, added everything in supporting the soft parts drawn in front, without the slightest inconvenience or pressure from behind, so completely had it

been modelled for its position. The second needle was placed from left to right above the red margin of the lip, entered fully three-quarters of an inch beyond the cut surface, was carried from left to right with a slight curvilinear course convex upwards, so as to depress the flaps to a corresponding point on the opposite side. A long silk ligature was evenly cast around this needle in the same figure of eight fashion, and it was astonishing to see how readily and evenly the parts came together, the cheeks being well pressed forward before the thread was at all tightened—in fact, as I have stated before, the soft parts were stretched forwards on either side on the needle until in contact accurately, and then the cord was cast over the needle in the figure of eight form, so that the adjusted surfaces could not retract. A third needle was inserted between the upper and lower, and at the same distance from the cut surfaces; it was passed exactly in the same way from left to right, and including the same depth of the lips close to the mucous membrane. A ligature, in the same figure of eight form, was thrown around it, the parts pressed forwards, and the ligature tightened, and in several turns made to pass around the needle, the entire wound was most accurately brought together—most evenly every way. The points of the needles were then clipped away. On examination, nothing could be more perfect than the way in which the septum was formed, and lay with the side parts accurately and with the greatest evenness together. The red margin was perfect and the intervening part between the upper and lower needle steadily in apposition. The wide cheek supports forward, cut of sufficient length from the soap plaster, were next applied as in the foregoing case, from temple to temple beneath the chin steadying the cheeks and cut parts well forward, and so resisting any attempt at retraction. A long narrow strap of plaster was passed from the right cheek upwards, and in semi-circular way over the nose to a similar position on the left cheek, so as to restrain down the nose, and in this way take all strain off of the little central piece forming the covering for the septum until perfect union should be established. The woman was removed to bed, got a full opiate, and gently went to sleep. She soon partook of some strong beef tea; ordered it and as much milk as she pleased to drink; a full opiate three times a day, in short, the woman was half narcotised as in the child's case. Thus she went steadily on until the fourth morning after the operation—*ninety six hours ;* when I removed the needles with the same manipulation as in the child's case; the upper needle first, that above the red border next, and, lastly, the middle one. The three sutures, being perfectly agglutinated to the lip, were left undisturbed. Three long straps of plaster hollowed a little on either side towards the centre, with a longitudinal slit in the middle, were passed from the right cheek across to the left, supporting the parts throughout ; a fresh cheek supporting piece was applied as before.

During all this manipulation the cheeks were steadily pressed forward by an assistant from behind. The long narrow strap was also replaced over the nose. On looking into the mouth the lip seemed perfectly healed, and so also the red border.

On the following day, the fifth after the operation, the dressings were all removed, and now the sutures came away too. Nothing could be more satisfactory or perfect throughout than the way in which the cut parts united in every part; the septum, so troublesome to form and so difficult to adjust the several cut pieces, lay most admirably and steadily joined, only a trace of discharge from within the nose, while the central line of the lip was healed from top to bottom, and the line of union not thicker than a fine thread. The junction of the red border was perfect, not the slightest gap; the lip was straight across and prominent below, owing to the curves above which, when brought accurately together by the centre needle, protruded it to the natural conformation. All the dressings were put on with the same carefulness as before, and every day for ten days a similar proceeding was had recourse to. The needle holes were all now obliterated, the little discharge ceased from the nose, and, certainly, a more perfect operation I never performed. The whole aspect of the woman was altered; from being the most revolting object to look at, she really became a well-looking woman. Nothing was more remarkable than her astonished look when she sawherself in the glass— the tears rolled down her cheeks with joy.

There was another great object gained besides the improved appearance of the woman by this successful operation. Previous to it her efforts to try and speak were most distressing to witness, the convulsed state of her face was truly frightful, and not a gutteral sound that she uttered could be understood, while in three weeks after the operation she could speak long sentences quite clearly, and with every word distinct and expressive. I was quite astonished at this remarkable change, and so quickly brought about. Three weeks after the operation I had a most admirable photograph taken at Mr. Robinson's establishment, of the woman and child in the same attitude exactly as had been taken before the operation. Plate XIV. gives an accurate representation of the front face of the mother and child, while Plate XV. gives the profile of both mother and child most perfect in every way, and with most favourable contrast with those taken before the operation (see Plates XII. and XIII.). Nothing can be more accurate than the way in which the photographs have been traced and reprinted by Mr. M'Connell, in the beautiful sketches which he has produced.

In my work on Operative Surgery there will be found described, and practically illustrated by cases, several ways of dealing with the projecting central piece, according to its size, bulk, and

PLATE XIV.

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.
DOUBLE COMPLICATED HARE-LIP IN MOTHER AND CHILD,
FRONT VIEWS AFTER OPERATION.

PLATE XV.

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.
DOUBLE COMPLICATED HARE-LIP IN MOTHER AND CHILD, SIDE VIEWS AFTER OPERATION.

direction, but the methods which I have described were most suitable to the cases of the mother and child which I have just recorded.

CASE IV.—*Ununited Fracture of the Leg—Fracture very oblique through the Tibia—Subjected to various forms of Treatment without any beneficial result, and ultimately Cured by pegging the bones with ivory nails—Perfect Recovery.*— William Cooper, aged thirty-six, a carpenter by trade, consulted me early in June, 1874. He stated that, in getting into a tramcar at Rathgar, he fell and had his left leg smashed, the 24th of December, 1872, at 10 p.m. He was tipsy, and tried to get into the car at the front when it was moving; he was thrown down, and the leg broken very obliquely; the fracture through the tibia was very oblique from above downwards, and from the outside, for fully four inches, commencing about its centre, the fibula was smashed in pieces. He was removed to an hospital, and was under treatment in it for twelve weeks; at the end of this long time there was no union. He was sent to the Loughlinstown Convalescent Hospital to improve his health for six months—all to no effect, though plaster of Paris and starch bandages were had recourse to, and occasionally forcible friction of the fractured bones upon each other, were all tried in vain. The patient had to move on crutches, and could not bear his weight on the foot; at his attempt to do so the leg at once yielded, and showed a remarkable angular deformity at the line of fracture. The patient was admitted under my care to Sir P. Dun's Hospital the 10th of June, 1874. He came into the ward on crutches as described; the soft union was quite evident; the bones could be moved fully more than one inch in contrary directions; the line of fracture was very oblique, and fully four inches long; the lower end of the upper fragment of the tibia projected sharply above and over the lower fragment of the broken tibia for fully two inches.

On the 25th June I passed a strong double-edged very narrow-bladed knife, not more than a quarter-inch in width, by subcutaneous section, between the bones, and cut up all the ligamentous tissue between the ends of the bones; this proceeding was very troublesome, yet executed without much loss of blood. I then made three small incisions, about a quarter of an inch long, at intervals of an inch, over the projecting fragment of the tibia, the lowest being just at the end of the upper fragment; I drilled then three holes through the upper fragment—the two lower ones went also through the lower fragment of the broken tibia—and I drove down with a mallet to the heads three ivory pegs fully two inches long—these steadied at once the two fragments; then a few straps of plaster between the pegs and lint soaked in oil completed the dressing. The limb was then placed in my box for fractures of the leg and ankle (Butcher's Operative Surgery, p. 538), and effectually

steadied by footboard, pads, and side splints, above and below, leaving the part operated on exposed to view (see Plate XVI.).

July 12th.—All going on most satisfactorily; no pain of any amount or excess of inflammation.

August 4th.—The pegs perfectly firm still, and the limb quite steady and solid; put an additional ivory peg into the lower fragment, by drilling close to the extremity of the upper fragment, with the object to set up an active inflammation in this weakened point, and so further the throwing out of an additional supply of lymph, to be developed into cartilage and bone.

7th.—A great deal of pain and inflammation originated from the introduction of the last peg, which, however, gradually subsided, and was followed by the most salutary results, for, on the 20th, the limb was nearly quite firm and *the pegs quite firm.*

Sept. 10th.—The limb is quite firm; pus wells up from the peg holes, and yet each peg holds its place with a steady grip; the patient does not complain of pain; the limb has been bound up for several days, subjected to considerable pressure, and most particularly about the broken ends of the bones.

18th.—Going on most favourably, and union very firm.

Oct. 28th.—Union quite solid; removed the pegs, an act which was accomplished with considerable force; each had to be gripped with a tooth forceps, and rotated, and steadily with force drawn out. Neither of the pegs were in the slightest degree eroded or lessened in circumference. The second peg was not so tight, the parts around it being evidently slightly absorbed; and the proof of this, that the peg was not even roughened or absorbed, it fitted tightly a hole made by the same drill as that for the peg at first, so that the statement copied from book to book, that the pegs are absorbed and lessened in diameter, is not correct. On removing the pegs the fractured bones were found to be perfectly consolidated; the wounds were dressed, and the limb bandaged and allowed to rest in the box until all was healed.

Nov. 15th.—Up every day; had some difficulty, after so long a confinement in the recumbent position, to retain an elevated one, as fainting threatened, and, in one instance, absolutely supervened at one time in a persistence to maintain it; however, by cautious stages, this serious complication was averted, and the man is now able to sit up all day.

27th.—Able to go about on crutches; wounds all healed, and on progression the leg rests well down to the ground.

Dec. 5th.—Extracted a small piece of bone, and since then, now the 7th of December, the wounds all healed up, the patient moving about with crutches, and leaning considerable weight upon the limb.

Feb. 18th.—Now able to walk without stick or crutch; limb quite firm, and dismissed the hospital quite well.

PLATE XVI.

Drawn from a Photograph.

MR. BUTCHER'S REPORTS IN OPERATIVE SURGERY.

ON UNUNITED FRACTURE OF THE LEG, SUCCESSFULLY TREATED WITH IVORY PEGS.

Owing to the indiscretion of the man, he was readmitted March 24th with phlegmonoid erysipelas of the same leg of the worst description, brought on by the most irregular habits and marked intemperance. Matter had formed from the intensity of the inflammation, and had to be cut for in several places—on the inner side of the limb, and again along the inner border of the tendo Achillis; several sloughs were discharged, and a large quantity of pus let free. Bark mixture, beef-tea, and eight ounces of spirit.

29th.—Somewhat better; discharge very free, yet, with all this mischief around, *the union remains quite solid.*

April 19th.—The limb is going on quite favourably, and the sinuses healing rapidly, and general health steadily building up.

26th.—Nearly all healed; health repaired.

Since a small portion of dead bone can be felt, and remains still buried, yet harmless to the limb; a narrow channel leads to it, and a like small discharge from it. The limb is solid, perfect for progression and hard usage, to both of which tests it has been amply submitted. The entire health and appearance of the man have been wonderfully restored, and he is now able to walk without stick or crutch, and to earn his bread as he was always able to do by his trade as a carpenter.

I have been greatly pleased by the successful result of this case, as by several surgeons and many appliances his cure was attempted, yet all ending in discomfiture, discompletion, and failure. The more scientific and decided measure, as described, will be received by practical men just as the result conveys instruction and sound information in a thorough consummation of the wishes of the surgeon.

I have accurately and rather minutely described this case, because it has been so recently operated on, and because I have been able to follow it up in all its relations to the present time, proving how salutary the efforts had recourse to have been. The man is now able to walk miles, and to earn his bread by persistent work and industry. The symmetry of the limb and its proportions have been preserved, and the steady position of the man, as he rests his weight equally on both limbs, proves how efficiently the one operated upon maintains its own functions.

Dec. 8th, 1876.—I have this day seen the patient at Sir Patrick Dun's Hospital; he wished to show me how well the leg was, more than seven months having passed since he was under my last observation, and for several months past, as now, he is able to work at his trade as a carpenter without the least impediment.

In the foregoing cases operative surgery has taken its place as predominant, yet I will now recount a case of the gravest nature, the deepest interest, in which, if I may use the expression, medical surgery stands in a conspicuous way forward.

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CASE V.—*Fracture through the Base of the Skull from great violence applied to the Vertex—Traversing the petrous portion of each Temporal Bone and the Bodies of the Sphenoid and Ethmoid, with profuse Hæmorrhage—Recovery.*—John Carroll, aged twenty-six years, a powerful young man, admitted into Sir P. Dun's Hospital, under my care, January 31st, 1876. The following is the history of this most remarkable case :—The man was unloading some large beams of timber, when the end of one of them swung from its intended position, and struck him forcibly on the top of the head. The man was knocked down several feet from where he stood, perfectly insensible. In this condition he was brought to hospital, when I was summoned to see him. He lay in bed perfectly insensible, with panting respiration, a miserable pulse, covered with cold perspiration, and, as it appeared, dying. He was bleeding profusely from both ears, and blood was in his mouth, and forced between the teeth. On careful examination of his head, the vertex was bruised to the extent of the size of the palm of the hand—no bruise, or wound, or signs of force being applied anywhere else. From all the circumstances, I had no doubt he suffered from fracture of the base of the skull, traversing the sella turcica and petrous portions of the temporal bones. The blood from both ears and that from the nose and mouth confirmed this view. The man lay panting for life ; the skin cold and moist, with the pupils widely dilated. A large turpentine fœtid enema was administered, and heat applied to the surface of the body and along the limbs; six grains of calomel, mixed with a little honey, was placed upon his tongue, and some warm beef-tea injected into his stomach. On the evening after his admission his pulse slightly got up, and his head became hot; the hair was at once shaved off, and ice-bags ordered to be constantly applied. On the following morning, Feb. 1st, the patient was perfectly warm, and reaction was beginning fairly to be brought about. The man lay in the same unconscious way, and could not be roused to speak or answer questions. An hour before my visit he vomited copiously fully three-quarters of a pint of clotted and fluid blood which had escaped from the nose. There was no further bleeding from the ears ; the head was hot, and the pulse getting up ; ordered the head to be shaved, and ice-bags constantly over the head, and six leeches to each temple, and after their falling off, the trickling of blood to be encouraged by warm sponges ; ordered one grain of calomel and one of James's powder every third hour, with a fourth of a grain of tartar emetic ; urine drawn off by the catheter. The man could move the arms and legs perfectly, showing there was no brain laceration or paralytic affection. On the following morning, Feb. 1st, he was still in the same unconscious way ; the heat of the body greatly increased, the face flushed, and the head much hotter ; ordered ten leeches to the temples, ice to be kept constantly applied to the head, powders to be continued every fourth hour, and a

full fœtid enema to be thrown up. It was scarcely retained more than four minutes when it passed away, together with some urine from the bladder.

Feb. 2nd.—Lies much in the same unconscious state; his pulse had got up, and the whole circulation hurried, and, as a consequence, some fresh oozing of blood from the nose; ordered tincture of digitalis in small and repeated doses through the day, which effectually lowered the heart's action and restrained the bleeding, in conjunction with ice constantly to the head; six leeches were applied to each temple, and the calomel and James's powder continued as before.

Feb. 3rd.—Has never once opened his eyes; he lies quite unconscious, even when spoken loudly to; he has vomited a large quantity of blood from the stomach, of a dark colour, evidently changed by the gastric juice, while that which flowed from the ears was of a bright scarlet colour; a large quantity of the same scarlet blood flowed through the Eustachian tube into the mouth and pharynx, and so into the stomach; the movements of the man's limbs much better, and the man suddenly tried to force his way out of bed with the intention of going to the night-chair, but had not the power, and fell back in the bed, passing freely from the bowels and bladder their contents. All this time the patient's eyes were closed, and he seemed perfectly unconscious of what he was doing; he had not the least power of speech or consciousness of what he had been doing. He showed the most marked reluctance to open his mouth; the efforts to feed him were so wearisome, as he would not swallow, that beef-tea was thrown up by the rectum, so as to sustain the powers of life. The calomel and James's powders continued every fourth hour, together with the application of eight leeches to each temple, and constantly fresh ice-bags to the burning head. Symptoms continued much in the same manner until the 4th of February, when the pulse rose, heat of head increased, and the man became absolutely violent, and could not be held in bed; he had a violent convulsion; the mercury was then pressed more freely; ordered also eight leeches to each temple, cold to the head, and a large blister to be applied to the occiput and nape of the neck, to be left on for eight hours, and then the vesicles to be removed, and the surface dressed with mercurial ointment. Now the patient could drink milk, and with avidity, which was his chief sustenance. Towards evening he became quieter, but made no effort to speak. There was no return of the bleeding from either ears or nose. At this stage, and from the prominent symptoms, no doubt was entertained that inflammation had, in spite of all the antiphlogistic measures employed, attacked the membranes of the brain, and that in a very sudden manner. However, the remedies had proved successful in checking the more violent exacerbations. The ice to be continued, the calomel and James's powder also; milk and water iced, of which the patient drank abundantly when put to his mouth, though he scarcely separated the teeth.

Feb. 5th.—The patient lies very much in the same state; he has had three convulsive fits, supervening rapidly one on the other, his head getting sometimes very hot, and starts up suddenly and makes efforts to get out of bed; pulse up, and very strong. He makes no effort to open his eyes but when lids are forced apart; pupils contracted to smallest point, which state they maintain ever since the third day after the accident, previous to which, and from the first, they were widely dilated and insensible to light. When the effort to open the man's eyes is made, so irritable is he that he crouches beneath the clothes, burying his head under the pillow. Ordered to continue the powders still every third hour; no mercurial effect as yet evident; eight leeches to each temple, and ice to be applied constantly as before, also the digitalis. 3 p.m.—Circulation much reduced in force by the repeated doses of digitalis and the copious discharge of blood by the leeches, the ice also being unremittingly applied. Ordered a large blister over the right tempero-maxillary articulation; and on the cuticle being cut and removed, to be dressed with mercurial ointment; the blister on the back of the neck freely discharging; bladder and bowels empty themselves, but without any notification to the patient; to continue all medicines, &c. Matters continued much after the manner described—exacerbations and relief, violence and quietude, according as the several violent antiphlogistic measures were enforced. Up to the 11th, when the man became much quieter, no repetition of convulsions; the pulse softer, and deprived of its wiry beat; the head cooler, and the skin of the body moist. Now the mercury produced its all-salutary effect, ptyalism was perfectly established, and so the chain of action by those several powerful antiphlogistic measures was perfected.

Feb. 12th.—The patient had quiet sleep for an hour or more together, and began for the first time to look about him—only for a few moments, however, as he would again relapse into a dozy state. Pulse soft and compressible, yet his head sometimes very hot. A most unaccountable restlessness at intervals takes possession of him, and when he quietens, he gazes vacantly for a few seconds, and shuts his eyes again; cannot be induced to open his mouth; sucks in quantities of milk, on which he has lived since his admission to hospital; to continue mercurial powders twice a day, the mercurial dressing to the blistered surfaces, and a large blister to be applied to the right temple, the shape and size as ordered a few days before for the left temple; ice still to be applied at intervals to the head when the heat-flushing comes on, or unusual restlessness; omit the digitalis; all other remedies continued.

Feb. 16th.—Very little alteration in the management of the case for the last four days; the mercurial has been continued, and the blister on the neck and left temple renewed; the milk diet has sufficiently sustained the patient.

Feb. 20th.—The patient has gradually improved since last report; he looks about and smiles, and makes an effort to speak, but cannot; will not make an effort to open his mouth; the pulse is soft, and 78 in number; skin moist; head cool; towards night inclined to get hot, which is productive of some restlessness, yet the ice-bags check the fulness, and in no instance now does he become unruly. He also has restored consciousness now as to ask for the night-pan or urinal when he requires either. Ordered one powder at bed-time, and the mercurial dressings to the blisters on the neck and right temple; the left to be again blistered as before.

Feb. 22nd.—Gradually mending; mercurial fœtor only faintly perceived for the last few days, yet for many days it has been silently working; the beneficial changes steadily progressive up to this date; he makes the effort to speak, but quite indistinctly; pupils restored to natural size, and now sensible to the stimulus of light; sleeps quietly, and all troubles over; vigilance and restlessness have passed away; if restlessness and vigilance supervene, the ice-bag speedily, after its application, induces sleep. One powder at night now continuing the effect of the mineral, and ptyalism of a salutary form is steadily maintained by the prescribed doses and the absorbing surfaces upon which it acts.

Feb. 24th.—The man's condition since last report has much improved; he is quite conscious of everything passing around. When spoken to, he brightens up and smiles, and can speak and answer a few words. His pulse are steady at 72—quite soft; his skin moist, and the bowels and bladder acting healthily, and both under full control of his will. He spoke for the first time to-day, " What brought him here—he would like to be up." This day, for the first time, I could, with a little gentle pressure on his chin, open his mouth almost a quarter of an inch, beyond which he would not allow.it, and foamed and struggled to get back upon the pillow, and seemed in pain after the effort. It was quite clear that the slightest motion of the lower jaw disturbed and pained the fracture through the bones. The man has full power of motion of the upper and lower limbs, and in all sensation is perfect; yet, with all this, he is careful in moving his head, and will not allow himself to be handled about the angles of the jaw or the auditory foramen. The mercurial dressings to be continued; the one pill at night, and a fresh blister to the nape of the neck; beef-tea and milk diet.

Feb. 27th.—Has gradually improved; more conscious, and comprehends the meaning of questions more clearly. His pupils are natural, and act well to the stimulus of light; pulse soft and compressible, yet the least excitement in endeavouring to answer questions sets the head going, pulsations of the carotids, and flushed cheeks; says " he does not know what is the matter with his head, the pain is very great sometimes, and he is bothered." On making pressure over and beneath the mastoid

processes of the temporal bones, great pain is occasioned, at the same time the face at once becoming more flushed, and intense pain occasioned from ear to ear, and, as the patient expressed himself, " it was straight up to the top of his head." Bowels acting steadily, and urine in full quantity. The patient now gets up to the night-chair, and has so far recovered sensibility as to provide for his own wants. Ordered a large blister to the nape of the neck, and bromide of ammonium in twenty-grain doses, with hyoscyamus, three times a day; ice occasionally to his head; beef-tea and milk diet. The symptoms and conditions manifested to-day very strongly corroborate the conclusions arrived at as to the line of fracture. As yet there is tenderness and delayed security in the whole line of fracture.

March 2nd.—Is gradually becoming more gentle—he speaks in quite a subdued way, with a smile on his face; his mind does not appear quite clear; and on being spoken to, and he making the effort to answer, he at once flushes up, and becomes confused; the head frequently becomes hot and pained, and then he complains to the nurse, and the ice-bag is applied, which soon calms him down and allays all pain. The blister is kept open at the back of the neck. Ordered milk and beef tea, and a little bread for support. Pulse soft, very compressible, and in number 84.

March 10th.—Permited to get up; his whole body is greatly wasted, and he feels unable to stand almost. When asked to look around he guardedly avoids rolling his head, but turns round his shoulders, with the head quite steady, and the muscles of neck rigid; no doubt they exert a protective influence in this way to prevent any disturbance of the broken bones. He speaks in a kind of tremulous silly manner still.

March 25th.—Has been walking by degrees about ward ever since last report, and is gaining strength, and also decision of character in a marked way; he can now also turn the head without check or hindrance, and the rigidity of the muscles has also quite disappeared. After this the patient was sent for some time to the Convalescent Home, and soon after returned to his friends quite well.

The foregoing case affords a good illustration of the practical rule—*no matter how serious the case or complicated the injuries may be, every effort should be made to save life.* When this man was admitted to hospital nothing could be more hopeless than his condition. Knocked senseless by a violent blow on the crown of his head from the swing of a huge beam of wood, bleeding profusely from both ears, nose, and mouth, and life nearly extinct, it was quite clear from the first that the force transmitted from above downwards to the buttresses of the arch was so great as to fracture the base of

the skull, the track most likely being through the petrous portions of each temporal bone, and through the sella turcica; in this way the ruptured vessels bled freely through each external meatus, and through the bodies of the ethmoid and sphenoid bones. The quantities that flowed by the nose, ears, and mouth was very considerable, but not greater than that vomited on the evening after admission. On the third, fourth, and fifth day was shown the activity of the mischief, in spite of all the very active antiphlogistic means employed to check it. Then again, at a later period, the violence of the man to get out of bed and dash himself about, showed the tenacity with which the inflammation of the membranes and the brain itself endeavoured to advance; the details of treatment, the repeated local depletions, the depressing agency of digitalis on the heart, the steady and continuous use of mercury both internally and externally, the combination with the mercurial of larger and repeated doses of James's powder with small quantities of tartar emetic, the oft-repeated applications of blisters to the nape of the neck, the temple, and the head, and the assiduous use of cold, have all been accurately recorded from time to time throughout the pages of these remarks; and I may add, nothing could by possibility have saved the patient's life but the assiduous way in which all my directions were carried out.

ART. XV.—*On the Embryogeny of the Intestinal Canal in Man and in the Higher Animals.*[*] By ALEXANDER MACALISTER, M.B., Dubl.; Professor of Zoology in the University of Dublin.

IN my last lecture I endeavoured to lay before you some of the preliminary stages of the development of the ovum, in order to explain the earliest traces of the mesoblast, and the muscular apparatus developed therefrom. To those of you who followed my demonstration it must have been obvious that I passed by without comment one important and considerable set of changes—namely, the conditions intermediate between the formation of the cavity of von Baer and the formation of the central cavity of the blastoderm. We left the former abruptly when it appeared as a space filled with fluid derived from the liquefaction of the central cells of the morula,

[*] A Discourse delivered before the King and Queen's College of Physicians in Ireland, on Monday, February 12, 1877, being the fourth of the Annual Scientific Lectures for 1877.

and we at once leaped to the stage where the embryo showed us a central cavity with a bilaminar wall.

It would be a natural question to ask—Are these two cavities the same? is the cavity of von Baer the same as the central space which is becoming the digestive canal? And the older embryologists gave to this question a positive affirmative answer, as in the egg of birds and mammals there appears no other cavity than the one which underlies the newly forming hypoblast, and the walls of it appear to form by the process of delamination or the cleaving into two or three of a primarily simple layer.

In the simplest of all vertebrates, however, the amphioxus or lancelet, the egg sphere at first appears as a simple one-layered blastoderm, which, thickening below, becomes folded in, and thus, by a process of invagination, becomes secondarily bilaminar, the segmentation cavity becoming obliterated by the approximation of the fundus of the invaginated pouch with the inner circumference, and thus, in this case, the hypoblast is formed as an in-growing process from the primarily simple blastoderm.

In the lampreys—the group which, in order, succeeds the lancelets—the hypoblast is formed by a similar invagination, as can be seen in the diagram. In amphibians—such as the frog—the early changes in the development of the hypoblast are of the deepest interest, and, as they have been very extensively observed, we are quite familiar with them now.

In the egg of the frog, when segmentation takes place, the portion of the yolk which is rich in food material, lies below the embryonic part of the ovum, cleaves much more slowly and into larger masses, which come to the surface of a limited area of the inferior part of the egg, where they have been known as Ecker's yolk plug; between these larger cells and the true blastodermic cells, a fissure appears, which is known as the fissure of Rusconi, after its first describer; this fissure dips in between the line of embryo cells and the food yolk cells, becomes lined with the cellular elements which it carries before it in its extension, the true segmentation cavity is pushed aside and diminishes, and ultimately this space becomes a large definite area, lined with hypoblast, the primitive alimentary canal.

Thus in the amphioxus—in the lamprey and in the frog—the formation of the intestinal canal is primitively a process of invagination or of involution, but as the quantity of food-yolk increases, so does the lower part of the embryo diminish in the activity

of its growth processes, while the upper part becomes more active, and hence, in some of the higher amphibians, there is the singular condition to be noticed that the cavity seen in place of the primary segmentation cavity has, from the first, a thick wall composed of more than the epiblast, of what is about to be functionally a hypoblast, and that between its cells and those of the coarser food-yolk a fissure appears, bounded only above by distinct cells, below by food-yolk masses, and this space becomes an alimentary canal by the extension downwards of its upper wall.

In birds and mammals the so-called segmentation cavity, from its first appearance, has a lining of hypoblast, which appears as speedily as differentiation sets in in the embryonic area; but which, from the first, exhibits all the appearances seen in the advanced amphibian embryo, and this first seen cavity itself becomes developed into the intestinal canal.

On comparing all these stages, there are only two conclusions at which we can arrive—either the bird and mammal have their primarily digestive systems developed in a way totally unlike any of the other lower forms, while they are apparently tending towards the same conditions, or else in the bird's egg the cavity within the hypoblast is the remains of an invagination cavity, and that process has taken place during the earliest stage of cleavage, so early that its products and the products of cleavage appear simultaneously. When these alternatives are presented to any reasonable mind, there is no doubt in which direction the weight of evidence tends, so that we may regard it as in the highest degree probable that in all vertebrates the hypoblast is formed by a process of invagination, and the primary alimentary canal is the cavity of the invaginated sac. This, which is demonstrable in the eggs of fishes and amphibians, has been shown by the researches of Metschnikoff, Salensky, Ussow, Kowalewsky, and many others, to be true among invertebrates. So that we may assume it was universal in the animal kingdom.

There is also a singular point of collateral evidence, to which I would here allude, as being, in the strongest sense possible, confirmatory of the theory of the embryogeny of birds and mammals just proposed—that in these the layers have been formed by the same processes as in fishes, amphibians, and invertebrates, but that the early stages are passed over simultaneously with commencing organisation. The point is as follows:—In the lower forms, fish and amphibians, the embryo lies, at first, at the edge of the blasto-

dermic area of the egg, and the neural and alimentary canals, being both formed by invaginations at two sides of a common membrane, communicate with each other behind, and the two canals are comparatively late in being separated one from the other, the separation being due to the atrophy of the hinder part of the digestive canal. When development advances, the lateral growth of the blastoderm alters the position of the embryo, bringing it ultimately into the centre of the disc, and during this process the extending embryonic folds grow on each side of the middle line of the embryo, gradually encroaching on it until they medially coalesce, the groove between them disappearing.

In the birds and mammals' embryo, the embryo from the first appears centrally in the blastoderm, but coincident with its appearance there is seen at the hinder two-thirds of the blastodermic area, extending from what is to be the centre of the embryo to the edge of the area, a streak, furrowed by a groove on the upper surface, which becomes first prominent and distinct, and then becomes less and less distinct until it has completely vanished; its disappearance is complete at the close of the second day of incubation. This groove, long confounded with the after-forming medullary groove which is the origin of the nervous system, and from which it is perfectly distinct, is an appearance which has been shown to exist in mammals and birds, and the only point of importance noticed about its structure is that the epiblast and hypoblast are united along its floor.

Comparing the early stages of the mammal and bird embryos with the primitive groove in existence, and the more advanced stage of the fish and amphibian embryo, what do we see? That, as in the latter, the embryo is shifting from its peripheral to its more central position, the expanding edges of the blastodermic area in spreading come together medially, coalesce and form a fusion, that under this the epiblast and hypoblast are perfectly continuous, their tubular infoldings, neural canal, and alimentary being continuous, while in the former we just catch the glimpse of a precisely similar groove and a precisely similar fusion in the act of vanishing. Surely these appearances are inexplicable on any other hypothesis than that which would regard the embryo of the bird or mammal as having passed through a stage of alteration in position simultaneously with its first formation, and taken in connexion with the former consideration of the formation of the alimentary cavity, the evidence is thus cumulative in favour of the proposition that the

egg of the higher vertebrates has passed through, in its development, stages of existence comparable in nature with those of the lower vertebrates and invertebrates, stages which, in point of function, leave no traces whatever in after-existence. I refrain from indicating to what the change in position in the lower vertebrate embryo is due, as, though interesting, it is beside our present study.

What a wonderful simplicity, then, is there in these early stages of embryogenesis. A one-layered sphere, by invagination, becomes bilaminar. The central sac of this form, by the outgrowth of diverticular processes, extensions of the continuous lamella, becomes surrounded by a body cavity, and its wall becomes the muscular system. Another folding-in forms the neural canal, and thus the whole embryo is resolvable into a unilamellar sac, folded in and out upon itself.

The after-history of the alimentary canal would be far too complex and wide a subject for us to trace in detail, so I will confine myself, in the remarks which are to follow, to three subjects:—1st, the methods whereby its apertures of ingress and egress are formed; 2ndly, the conditions which determine the after-divisions of the alimentary canal; and, 3rdly, a few considerations as to the development of its peritoneal surroundings.

As to the first of these, the methods of the formation of mouth and anus, I need not say much. The orifice of invagination never remains functional in any vertebrate, even the lowest; nay, it is only functional in coelenterates and turbellarian worms. It has failed to suffice for its duty, either for ingress or egress of material, and is supplanted by secondary orifices.

The alimentary canal, in its growth, extends fore and aft in the embryo, but is blind each way; however, at each end, involutions of the epiblast become depressed on the surface to meet the extending hypoblast, and the anterior aperture becomes the mouth, the posterior the anus. The place where the former opening arises is bounded by five lobes—two inferior maxillary, two superior maxillary, and one fronto-nasal. The place where the latter opening forms is posterior to that expansion of the lower end of the embryo which becomes the allantois. It is to be noted that in both mouth and anus there is to be recognised an atrial or epiblastic area interposed between the superficial orifice and the hypoblast proper, the mouth and buccal cavity in one case, the outer cloacal vestibule in the other.

Secondly, the differentiation of the alimentary canal into parts is due to the operation of two factors—relation and function. The

first region is surrounded with the visceral arches, and is perforated by the visceral clefts. This area is the pharynx, and is characterised by possessing the openings of the visceral slits, and is surrounded by the muscles of the visceral arches or their remains, altered into the constrictors of the pharynx. The second area is the supra-cardiac, compressed by the development of the heart below it, and constituting the œsophagus. The third, or post-cardiac region, is naturally divided, in development, into three parts in all the higher animals by the mode of formation of its lower wall.

As we saw in those animals whose eggs are mesoblastic, there is only a roof at first formed to what we now regard as the invagination cavity in the blastoderm, and this grows round by degrees in order to complete the lower wall. The fore-end early closes, then the hind, while the middle is slower in closing, and the mass of food-yolk which we know as the yolk-bag or contents of the umbilical vesicle communicates with this space. Thus there is a post-cardiac area of the digestive tube closed in early in front of the umbilical vesicle, a part open, communicating with this vesicle, and a part behind the yolk-bag closed into a tube.

The first of these differentiates in all vertebrates into two parts, from its hinder end, just in front of the yolk-bag, two sets of diverticula start, which become the liver and pancreas, and the part communicating with these—called duodenum or bursa entiana—becomes early marked off from the part in front of them, which dilates and becomes the stomach. The portion which communicates with the yolk-sac becomes elongated and coiled as the small intestine, while the post-vitelline closed region is the colon or large intestine.

Thus the pharynx, œsophagus, and duodenum are portions differentiated on account of their relations to other developing organs, and the other parts are specialised on account of the necessity, in the nature of things, for the fulfilment of two purposes—1st, the need of a sac as far forward as possible for the reception and primary alteration of the crude food; and, 2ndly, the need of a surface as extended as possible for the absorption of the nutritive elements, the forward position of the sac being necessary to give the needed extent to the membrane of the tube.

Closely connected with the consideration of the intestinal canal is the examination of its serous sheath, and to one or two points in relation to this we will devote the remaining few minutes of our time to-day.

The primitive intestine is slung to the vertebral axis by a meso-gastric fold, which contains a blood-vessel in its vertebral border, and this fold is reflected on the fore as well as on the hinder wall of abdomen above and below, and the higher vertebrate embryos has thus two median crescentic folds on the body-wall; the lower is the fold which passes on the pedicle of the allantois or the urachus, the other is the fold on the allantoic vein or the umbilical vein. Two conditions very early modify and complicate this simple serous arrangement—one is the development of accessory organs, the second is the elongation and extension of parts of the canal, and the necessary displacement resulting therefrom. Thus, the liver, developing in the parietogastric folds, throws back the stomach in its development, and interrupts that fold, dividing it into a pre-hepatic system of ligaments of the liver and a post-hepatic or lesser omental layer. The forward position of the liver is due to the early relation of development of its glandular substance with the network of veins opening into the sinus venosus. Then the stomach in its development is acted on by three forces—1st, its functional necessity to expand, which causes its enlargement; 2ndly, its anterior limitation by the expansion of the liver and heart on its forward aspect; 3rdly, its posterior limitation, due to the fixity of the duodenum, that condition springing from the attachment thereto of the glandular ducts. The result of these forces is that the axis of the organ becomes transverse, its vertebral wall becomes shunted to the left, and its primary anterior edge becomes superior, and thus, as its primary right side becomes turned towards the vertebral column, it pulls along with it its peritoneal clothing, which, being dragged over the fixed end of the duodenum, becomes the bag of the omentum; while the spleen, placed primarily in the mesogastric envelope of the stomach, is pushed over to the left side. The rapid alterations of distension and flaccidity of the stomach, occurring several times daily, drags the peritoneum from the neighbouring areas, and tends to produce lax folds in the neighbourhood of the stomach. The distension of the stomach being, from the necessity of its position, downwards, forwards, and to the left side, it is in that direction the lax fold projects, and as the peritoneal clothing of the back wall of the stomach participates in the pull, this lax fold or great omentum is bilaminar.

Thus far the formation of the omentum is a matter easily traced and easily accounted for.

The lower part of the intestinal canal elongates rapidly, and its

mesogastric fold grows *pari passu*, but the intestinal growth is not simple, as elongation takes place in two directions—1st, the small intestine elongates, growing chiefly downwards; and, 2ndly, the large intestine increases in size, though at a later period; but elongating upwards, as its growth takes place, it tends to grow upwards and to the left, the lateral direction being determined thus: the liver and its ducts keep the upper part of the small intestine to the right, so as the peritoneal roots of attachment of the upper and lower limbs of the canal are growing towards each other, the former passes the latter in its downward progress to the right, while the latter passes the other in its upward progress to the left.

But this growth cannot go on indefinitely, more especially as the two are continuous; the colon reaching its limit at the left side of the abdominal wall, by touching the spleen, is shunted across to the right, and as it grows it at first carries its own sheath with it until it has neared the right side, upon which it burrows into the root of the great omentum, and passing across touches the right wall of the abdomen, upon which it has to grow downwards, whereupon it extends, burrowing under the peritoneum as it stretches from the right of the mesentery to the abdominal wall until it reaches its maximum of growth by being jammed into the angle between the iliac fossa and the abdominal wall, whereupon it behaves itself in variable mood, sometimes endeavouring to scale the anterior abdominal wall, tearing most of the peritoneum from itself in the effort, or turning over the iliac artery and trying to become a pelvic viscus; in these attempts to make itself comfortable it pulls the peritoneum into numerous folds, and involutes it into pouches of almost endless kind and degree.

A very curious change in shape takes place in the cæcum of man in this progress. The early human cæcum is in shape fusiform, but, as may easily be imagined, from the time when it first meets with a difficulty in making its way, instead of its feeble point projecting its way forward, the big dilated base becomes the pioneer, and the feeble little tail slinks backwards, and thus from the time that, in the growth of the gut, the head of the colon has passed the mid part of the abdomen in its transverse ramble, the point of the cæcum streels behind it as a little tail, and by degrees as this condition prevents the entrance of alimentary material thereinto, it becomes smaller and smaller until, by the time the cæcum gets fairly on its downward way, this fusiform extremity becomes known

to us as the vermiform appendix—the puzzle of those sapient teleo-
logists who must find a use for everything.

It is interesting in this connexion to find that in those animals—
the wombat and chlamydophorus—that possess a vermiform appen-
dix, the peritoneum is arranged as in man.

Time fails to permit me to proceed further, but I hope I have
succeeded in showing how much light a rational understanding of
animal morphology throws upon descriptive anatomy by declaring
the reasons annexed thereto.

And we see in our studies of embryology that the embryo of man
is a palimpsest, inscribed over and over again with inscriptions not
easily decipherable; but, thanks to the guides and characters read
by those who have been for comparative embryology what the
Grotefends and Champollions have been for palæography, these can
be interpreted, and they reveal to us a history rich in the story of
failure, the record of plans tried and rejected, but richer still in its
story of progress—a progress through stages hemmed in by vary-
ing conditions, according to the operation of which, germs starting
from the one terminus have reached many and diverse goals.

ART. XVI.—*Notes in Electro-therapeutics.*[a] By WALTER G.
SMITH, M.D., Dubl.; F.K.Q.C.P.I.; Assistant Physician to the
Adelaide Hospital.

FOR many years subsequently to the first introduction of electricity
as a therapeutic agent, its use was entirely confined to its real or
supposed remedial powers, and its significance as a diagnostic means
was completely overlooked. It is not forty years since Marshall
Hall drew attention to the value of electricity in the diagnosis of
certain forms of paralysis, and, in the midst of much that was
erroneous, laid the foundation of a novel and important method of
investigation, which was afterwards extended by Todd and
Duchenne. Although much valuable work has been accomplished
in this department, especially within the past ten years, it is not
too much to say that its true import is only beginning to be
acknowledged by the profession at large. Hundreds of patients
are daily subjected to electrical treatment, for one case in which
electricity is employed as a means of diagnosis or prognosis. Yet

[a] Read before the Medical Society of the King and Queen's College of Physicians,
Thursday, April 5, 1877. [For the discussion on this paper see p. 473.]

it is almost superfluous to remark that without its aid we are un-
able to ascertain with precision the vital properties of nerve and
muscle, or to determine the exact state of nutrition in many
neuropathic and myopathic conditions, and in paralytic affections
especially it is indispensable in enabling us to form a just estimate
of the motor function.

. The case which I shall presently adduce derives its interest from
the fact that electrical examination led, at the outset, to the correct
diagnosis of a set of symptoms which had previously been other-
wise interpreted. To the patient this made the difference of
altering the prognosis from a prospect of increasing disability and
utter helplessness to one of certain improvement and probable
complete restoration to health.

CASE.—*Generalised Neuralgic and Paralytic Symptoms—Unsuspected
Lead Poisoning—Diagnosis of true cause by Electricity—Rapid Improvement
under Treatment.*—Thomas G———, aged fifty-seven, was sent to me
in January last, as a case of obscure nervous disease, with a view to
see whether electricity would be of any service to him. He was a
man of robust build, and reached fourteen stone in weight when in good
health. The following history was obtained from the patient and his
wife by Mr. Stuart Brooke, who took the early notes of the case. In
former years he had indulged a good deal in porter, had smoked freely,
and was fond of strong tea, but latterly he had been perfectly temperate
in every respect.

In August, 1876, the earliest symptoms of failing health developed
themselves. At first he felt his legs heavy, and experienced a drag in
the hips when walking. About six weeks afterwards he began to suffer
from pain in both arms, from the shoulder to the elbow, as also between
the shoulders, but had no pain lower down in the back. Meantime the
lower limbs were failing still more, and he found his duties very severe,
for he had to open and shut heavy gates between thirty and forty times
a day. He was now admitted into a hospital, and remained for
some weeks under medical care as a spinal case. Electricity was
applied along the spine and legs, and he was ordered a mixture of syrup
of phosphate of iron, solution of strychnia, and hypophosphite of lime.
This medicine was afterwards repeated by Dr. Ward, to whom I am
indebted for the opportunity of seeing the case.

After leaving hospital he went under a further special course of
electrical treatment elsewhere, but was soon compelled to give up work
altogether. When I first saw him at the Adelaide Hospital, January
20th, 1877, he complained of pain in the lumbar region of the back, and
was quite unable to walk without assistance. There was marked loss of

power in the four limbs, and the arms hung loosely by the side. He could not extend the hands, and the muscles of the forearm were atrophied and flabby, the wasting engaging both the flexors and extensors, chiefly the latter. The drop-wrist was symmetrical on both sides, but there was no perceptible wasting of the thenar or hypothenar eminences, and the fingers were not abducted. Although the loss of power over the extensors of the fingers was apparently complete, yet when the first phalanges of the fingers were supported on the edge of a thin book, or a piece of board, he was surprised and amused at finding that he could now flex and extend the two last phalanges with as much ease as a healthy person, thus demonstrating that the interosseous muscles were not paralysed. He was a right-handed man, and no undue stress fell on the left arm, yet it was the left arm and left leg which all through were the most crippled. No appreciable alteration or disturbance of sensibility was detected. His appetite was preserved; he was not constipated, and, except for the impairment of motility, was fairly well.

He was now subjected to electrical examination, with the following results :—The strongest induction current he could tolerate produced no effect on the extensors of the wrist or fingers, but the supinators reacted fairly, as did also the long abductor of the thumb (extensor ossis meta-carpi). Tested with a 20-cell voltaic current (Gaiffe-Clamond), the extensors of the fingers reacted slightly but distinctly, especially with a descending current, and interruption of the negative pole. My suspicions were at once aroused by these results as to the cause of the paralysis. No form of progressive muscular atrophy, of traumatic or rheumatic paralysis, or of central nervous disease which I had seen, presented similar features, and the only explanation which the symptoms seemed to admit of was that they were *due* to *lead*. Nothing in the man's occupation countenanced this view, but, on pressing him closely as to the posssible introduction of lead into his system, he stated as an unimportant fact that for two or three years he had fallen into a habit of constantly chewing small bits of lead "instead of tobacco," and indeed he was never without a small pellet of the metal in his mouth during the day, save when at meals. For some reason he had not chewed any for the past few months. He never imagined that any harm would come of this trick, and almost ridiculed the idea that such a trivial matter, as it seemed to him, could produce so much mischief, until he was shown the correspondence of his symptoms with those of an ordinary case of painter's drop-wrist which happened to come to the hospital.

Examination of the gums disclosed a well-marked and characteristic punctated blue line. He was at once ordered a mixture of iodide of potassium, sulphate of magnesium, and dilute sulphuric acid, and directed to attend regularly for electrical treatment. Within a week

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after commencing this line of treatment the patient was able to walk a distance of nearly two miles without much fatigue. The measurements of the arms on January 24th were—right forearm, 10 inches; left forearm, 9⅔ inches.

From this date he steadily and even rapidly gained ground, and by the end of a month had quite recovered the use of his legs. Lingering pains in the upper arms annoyed him for some time, but after a fortnight's course of medicinal and electrical treatment a marked improvement was apparent in the arms. Previously he could only with slowness and evident difficulty raise the arms and meet his hands at the back of the head, the hands being allowed to drop down by mere gravity, and the fingers gradually extended by traction on the head.

February 6th.—A temporary œdematous swelling appeared on the back of the left carpus, such as I have seen in several other saturnine cases.* On the 14th he was able to carry a cup of water to his mouth without spilling, although not capable of wielding a knife and fork for another week. The arms slowly increased in bulk, the muscles became firmer, and by the end of March he could partially extend the fingers and wrist, was able to oppose the thumb to all the fingers, and could write a good legible hand. While in hospital last autumn he was frequently tested as to his power of writing, but declares that he was utterly unable to pen a line " if he had got £1,000." Both faradisation and galvanisation were used in his case, chiefly the former, and, notwithstanding the great gain in voluntary motor power, little or no advance was noted in the excitability of the muscles towards induced currents.

He steadily persevered in the use of the prescribed combination of drugs, and took an intelligent interest in tracing the steps of his recovery.

On the 31st of March he affirmed that his legs were as strong as they were a year ago, although when he left hospital " he was quite sure he would be on crutches long before this." He was now able to lift eight stone from the ground, and could use his arms with considerable vigour.

The grounds upon which the primary diagnosis of saturnine poisoning was based were shortly these:—

(*a.*) The special implication of the extensors of the wrist and fingers, as shown by their loss of power, their atrophic condition, and their refusal to respond to faradisation. This latter feature is, I think, invariably met with in true lead palsy.

(*b.*) These muscles retained some degree of excitability, although a diminished one, to an interrupted voltaic current. In extreme

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* Various writers have observed the development of a dorsal tumour of the hand in cases of lead paralysis, as well as in traumatic paralysis of the radial and ulnar nerves, and in cerebral hemiplegia.—(*Cf.* Gubler, and Duchenne, fils, Gaz. des Hop., Mai, 1869.)

cases even voltaic irritability is lost; and it is certainly incorrect to state absolutely, as some writers do,* that, in lead palsy, irritability towards a slowly interrupted voltaic current is always *increased*.

(*c.*) The comparative exemption of the supinators. This is a curious, and at present, inexplicable fact in relation to the toxic action of lead, and one which I have repeatedly verified.

(*d.*) The immunity enjoyed by the interossei and lumbricales. It is only in severe phases of lead poisoning that these muscles are seriously impaired; whereas in Cruveilhier's disease they are commonly engaged, and indeed sometimes are the first tell-tales of the inroads of the palsy.

Before leaving this case, I wish to make two general remarks in relation to lead paralysis:—It is an error to state, as is often said, that, in the arm, the extensor muscles alone are damaged by the poison. In every case of pronounced lead paralysis, careful examination, manual and electrical, will, I believe, show that the flexors are also involved in a minor degree, as testified by their atrophic flabby condition and diminished reaction to faradisation. This view is, I find, also supported by Dr. Hammond. I think, also, that the doctrine often taught, that the extensors of the wrist and fingers especially suffer because they are the muscles most used, is too narrow, and does not fit in with all the facts. Three factors seem to concur in the causation of paralysis from lead, viz:—

(1.) The *specific toxic action* of the metal which usually, for unknown reasons, limits itself to the muscles of the upper extremities, but not very infrequently engages the lower extremities also, although it very rarely commences in them. This occasional implication of the lower extremities is one of the circumstances which might lead to an error in diagnosis.

(2.) *Pressure*, as illustrated in the wasting of the thenar muscles, and of the first dorsal interosseous in painters, owing to the pressure of the brush. In the case above narrated no palmar wasting occurred, because there was no local pressure.

(3.) *Fatigue*, from over-use of the muscles, as illustrated by the fact that in a left-handed painter the extensors of the left side will

* " In those muscles which are affected (in lead palsy), it is found that there is undue readiness of response to a slowly interrupted galvanic current and greatly diminished contractility (irritability !) to faradisation, and to a rapidly interrupted galvanism."— (Russell Reynolds, Clin. Uses of Electricity. Second Edition, p. 94.)

" In lead palsy the reaction to faradism is greatly diminished or abolished, and that to interrupted voltaism increased."—(Tibbits, Med. Electricity, p. 82.)

suffer most (Meyer); and that, in a hump-backed painter, whose lower extremities were weak, the paralysis especially attacked the muscles of the legs and feet (*ibid*).

The diagnosis between cases of lead palsy and other forms of paralysis is one of manifest practical value; and when, as in the above case, there was no obvious cause, and the paralysis was not preceded by, nor attended with the usual constitutional symptoms which belong to the infection of the system by lead, it appears that the electric reaction of the muscles and nerves furnishes a most valuable aid to diagnosis, and will often enable us to determine with certainty whether there be or be not lead in the system. Indeed, as Cyon remarks, "the most decided adversaries of electro-therapeutics have ended by themselves employing electricity as an element of diagnosis in affections of the nerves."

The establishment of the diagnosis of lead paralysis in doubtful cases, by the aid of electrical examination, has, so far as I know, seldom been made. This may be gathered from the quotation, in recent writers, of the solitary interesting case in point, which occurred in Meyer's practice, so far back as 1854 (Meyer's Med. Electr., translated by Hammond, p. 181). Without repeating the details, it is sufficient to say that, from a study of the electric condition of the muscles, Meyer was led to diagnose lead paralysis, although neither from the patient's occupation or mode of life, nor from any antecedent symptoms of illness, could a cause be assigned which justified this diagnosis. After a lapse of some months, further inquiries brought to light the fact that the patient had for many years used snuff, and this was *packed in lead foil.* Chemical analysis revealed the presence of a considerable quantity of lead in the snuff. After a protracted course of treatment, the patient ultimately recovered perfectly. (See, also, another instructive case of local paralysis, from snuff packed in lead, Meyer, p. 283. *Cf.* cases of poisoning, from the use of lead cosmetics, Report on Mat. Med., *Dub. Med. Journ.*, March, 1877.)

An important case was communicated by Dr. Althaus to the Clinical Society of London, in 1874 (*Trans. Clin. Soc.*, VII., p. 121). A chemist, aged forty-one, consulted Dr. Althaus for a paralytic affection of the left forearm, for which no intelligible cause could be given. From the peculiar electric phenomena, similar to those described above, Dr. Althaus concluded that the paralysis was produced by lead, although it appeared *primâ facie* unaccountable how the patient could have got the lead into his system

After many fruitless queries, the man suddenly mentioned that he had used, for the space of a month, large quantities of lead ointment (Ung. Plumbi Subacet. Co.), for dressing an extensive sore on his thigh. The diagnosis having once been settled, iodide of potassium was prescribed, galvanisation of the radial nerve and paralysed muscles was employed, and the patient made a rapid and complete recovery. Dr. Althaus truly remarks that the diagnostic value of faradisation was conspicuous in this case, for there was a total absence of other symptoms of lead-poisoning, such as a blue streak on the gums, dyspepsia, colic, or obstinate constipation; yet the faradic test proved at once that the case was one of lead palsy.

When commencing this paper, I had intended to quote other cases illustrative of the diagnostic and prognostic uses of electricity, but the length to which this communication has already extended warns me to conclude, and to reserve any additional material for another opportunity.

ART. XVII.—*A New Uterine Repositor and Retracting Speculum.* By ROBERT T. COOPER, M.D., Univ. Dubl.

THE readers of this Journal must have observed an error, serious but for its transparency, in the appearing of a wrong illustration in my paper on " The Removal of Foreign Bodies from the Ear," in the April number, where the second illustration in this paper, at p. 349, represents an instrument in every way differing from what might reasonably be supposed to be applicable as the pipe of a syringe, and certainly not representative of my description in the context. The mistake is one for which the instrument-makers are chiefly responsible, as, by an oversight, and unknown to me, they forwarded a wrong woodcut. This illustration, however, as it appears, is really one of a very handy and efficient instrument, designed by myself for rectifying malpositions of the womb, and which I have employed for some years; it goes with me under the name of a uterine repositor, and of late, by having it constructed in metal instead of vulcanite, and altered, as described below, I make it equally applicable for the purpose of a retracting speculum.

It will be seen that it consists of two arms, joined by a hinge, such that, by the aid of a stop upon it, extension is limited, so as to leave the arms forming a long, straightish instrument when opened out in one direction, while, in the other direction, rotation

is confined to allowing the arms to form with each other an angle
of about 55 degrees. In the former case the instrument serves in
malpositions of the womb as an eminently useful repositor; in the
latter, that in which the blades open at an angle of 55°, it forms a
retracting speculum, upon the same principle as Sims' duck-bill.
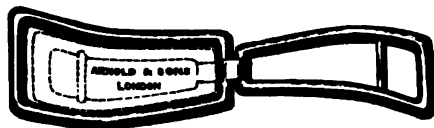
As a repositor the mode of using it is this—for antiversion, the
patient being placed in the usual obstetric position, the smaller
arm of the repositor passes up underneath the pubic arch lying
between it and the operator's right hand, his index finger acting as
a guide to the extremity of the instrument, by pressing against the
cross-bar placed about half an inch short of the end, and so im-
pelling it onwards; in this way the smaller arm of the instrument
is under the operator's entire control, the larger arm, owing to the
hinge-connexion, preventing the possibility of its becoming un-
manageable. The simple advantage of this repositor, therefore, is
that it gives additional length by half an inch to the operator's
finger, and its end being wide, as well as non-resisting, it allows of
the application of a steadier force to a larger surface than could
possibly be obtained by the unaided finger. The *amount* of force
required to effect the return of a misplaced womb is, I may observe,
in the great majority of cases, comparatively trivial, very slight
pressure in the required direction being all that is necessary to
attain our object. This arm of our instrument being gently curved
an inch from the end, will allow of our exerting pressure against
the fundus of the womb upwards and backwards—that is, in an
arched direction along the antero-posterior axis of the pelvis.
These particulars being explained, its utility in cases of retro-
version will be apparent—the only difference here being that
we will find it an advantage to direct our patient to lie on her
back, and allow the instrument to rest against the floor of the
vagina, the operator's hand with its index finger passing up in
front of the instrument. Thus we pass it in, resting it between
the hand and pubic arch for antiversion, upon the vaginal floor
for retroversion, and in either case it proves, in the great majority
of instances, an equally efficient instrument for the required
purpose.

The larger arm of the repositor will be found useful in aiding manipulation when returning a prolapsed womb, and, by the aid of a movable hinge-pin, we can render the instrument additionally serviceable in facilitating the return of an inverted womb, both of the blades being then employed in conjunction with the operator's hands.

I have had this instrument constructed with an additional cross-bar, an inch and a half behind that described, but which is not represented in the plate. This enables us to use the repositor for the purpose of a speculum, upon the same principle as we do Marion Sims' duck-bill speculum—namely, for exerting retraction against the vaginal wall, and so to allow of air, or, as Sims will have it (*vide* M. Sims' " Clinical Notes on Uterine Surgery "), the atmosphere, to enter the vagina, and thus induce an expansion of its walls. The patient, for this purpose, should, as Sims points out, be placed in the genu-pectoral or in a semi-genu-pectoral posture—that is, with her buttocks partially lifted up by the aid of some such prop as a couple of pillows placed underneath would afford. In some cases our retracting speculum possesses practical advantages over Sims'—thus, in operating for recto-vaginal fistula, it helps to steady the sides of the wound while the needles are being inserted; and, in cases of abscess of the walls of the vagina, it affords a ready means of exerting pressure against the walls of the abscess, while space is obtained for the discharge of its contents through the blade of the speculum; and, lastly, our instrument is extremely convenient, inasmuch as it is so portable.

REMARKABLE ANOMALIES OF THE AORTA.

LUTTICH (*Arch. d. Heilk.* XVII. and *Centralblatt*, No. 19, 1876) gives the particulars of two cases of aortic constriction. The first case was an obliteration of the aorta in the neighbourhood of the ductus Botalli, occurring in a healthy man of twenty-six, who had died suddenly by the rupture of a dissecting aneurism of the ascending aorta. The ductus Botalli was closed, close behind it the aorta to the length of 1·2 cm. was converted into a fibrous cord. Collateral circulation was maintained by the mammary, intercostal, subclavian, and carotid arteries.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Clinical Studies. Illustrated by Cases observed in Hospital and Private Practice. By Sir J. ROSE CORMACK, M.D., F.R.C.P.; Physician to the Hertford British Hospital of Paris. In Two Volumes. London: J. & A. Churchill. 1876. Pp. 548, 578.

A DISTINGUISHED graduate in medicine of the University of Edinburgh, and subsequently Physician to the Royal Infirmary and Fever Hospitals of that city, Sir John Rose Cormack has for some years past been in practice in the French metropolis, and now holds the post of Physician to the Hertford British Hospital of Paris. His name is familiar to the profession as a frequent contributor to medical periodicals, and as the able translator for the New Sydenham Society of Trousseau's *Clinique Médicale*. His noble and praiseworthy services to the sick and wounded during both sieges of Paris, as well as those of his lamented son, will not easily be forgotten.

In these two volumes of so-called "Clinical Studies," the author has collected a large number of papers and essays, most of which have been previously published, dating from his graduation (gold medal) thesis, which first appeared in print just forty years ago, down to the year 1875. The variety of the subjects treated of is one of the most striking features of the collection. Although the papers of later date are chiefly of a medical nature, we have here also dissertations on physiological, psychological, and pathological subjects ; accounts of capital operations in surgery, and of rare cases in midwifery; and papers upon venereal and allied diseases. The first volume commences with the well-known memoir on Relapsing Fever. This was published in 1843, and, while referred to by nearly every subsequent writer on the subject, is still probably the best description extant of the natural history of that disease. Reading it, we become at once as it were prepared for the author's future style, and feel that we are in company with a close observer and a faithful, if not a little prolix, reporter. Indeed, the minute-

ness with which daily, and sometimes hourly, reports of cases in this and other essays are given, tends after a while rather to weary the reader, and makes him apt to miss essential points in the "surplusage" of apparently unimportant details.

The second essay is on the subject of Cholera, and was the result of his study of that disease at Putney during the epidemic of 1848-49. If not a form of intermittent fever, Sir John concludes that cholera is a fever intimately related to those fevers which depend on malaria. The essence of the treatment of every case of cholera consists, according to the author, in arresting the serous discharge and in maintaining the warmth of the body by the application of heat externally. Under the caption of "Cholera Collapse: How Some Recoveries Take Place," is detailed a case in which the patient was allowed to drink as much water as he wished. Although the immense draughts swallowed seemed to be ejected as soon as taken, the water-drinking evidently gratified the instinctive craving for that fluid. Warm stimulant applications were made at the same time to the body. In a few hours there was a great abatement in his thirst and very little vomiting, and the serous discharge from the intestines were less abundant. Under judicious subsequent treatment the patient recovered.

The next succeeding chapters comprise a case of scarlatinous nephritis with abscesses in the kidneys, cases of puerperal convulsions, and a case of granular degeneration of the kidney in a scrofulous patient. In these papers the significance of the symptom albuminuria, as indicative of various forms of toxæmia and of renal disease, is ably discussed and well illustrated by the clinical details adduced.

A postscript, bearing date September, 1875, to a case of labour complicated with cauliflower excrescence of the uterus, contains some remarks upon a therapeutical agent but little known in this country—viz., the eucalyptus globulus. In simple uterine catarrh, Sir J. Rose Cormack says that he does not know of any remedy equal in value to preparations of this plant. "In such cases," he continues, "I have several times, with most satisfactory results, simultaneously administered them by the stomach and in the form of injections. As Gubler has shown, the anti-catarrhal virtues of eucalyptus are most remarkable. With increasing experience of its power I more and more employ it in bronchial, vesical, and uterine catarrh, in gonorrhœa, and in gleet." An infusion ($\frac{1}{2}$ ounce to 2 pints), or a tincture (1 ounce to 1 pint of rectified spirit) of the

leaves, or the essential oil given in capsules, are the preparations ordinarily employed. As a gargle or vaginal injection, and for external application, the infusion, or the tincture diluted (one drachm to six or eight ounces of cold or tepid water), may be used. Besides these therapeutic uses of the eucalyptus, the author adds his very favourable experience of its remarkable power of destroying the fœtid odour of morbid discharges *without the substitution of another unpleasant smell.* He speaks from an extensive trial of eucalyptus lotions in horribly offensive discharges in cases of ozæna, cancer of the tongue and throat, cancer of the uterus, gangrene, and other affections attended by fœtor.

In 1842 Dr. Turner published a paper in *The Edinburgh Monthly Medical Journal* upon the presence, in the recently-delivered woman, of a dark abdominal line or stripe extending between the pubes and umbilicus. Cormack soon afterwards discovered that a similar line often manifests itself in males, and in females independently of delivery. In the latter this pigmentation would seem to resemble the so-called chloasma uterinum.

In a chapter on "Dystocia from Cystous Kidney in the Mature Fœtus," are figured the appearances of a kidney the subject of this rare form of degeneration. A typical specimen, also causing dystocia, was exhibited by Dr. Duffey before the Pathological Society of Dublin some years ago.[a] And another probably similar case came also under the notice of Dr. W. J. Cummins, of Cork.[b]

The first volume concludes with an interesting *résumé* of five cases of hernia of the gravid uterus, and two cases of hernia of the unimpregnated uterus. These were all the cases of this very uncommon occurrence which the author could find recorded at the time of writing this essay (1841), and, like many other papers in both volumes, it bears witness to the extent of the writer's acquaintance with the medical literature of many countries, as well as to his power of amassing and arranging his material.

The second volume is, perhaps, the more interesting of the two. About one-fourth of it is occupied with the subject of "Air in the Organs of Circulation;" while the remainder is made up of strictly clinical cases—such as might be met with in daily practice—the interest and value of which are much enhanced by the author's suggestive and instructive commentaries. We have here the graphically detailed account of the case of reflex infantile convulsion

[a] Dub. Jour. Med. Sci. 1866.
[b] *Ibid.* May, 1873, p. 499.

treated by the hypodermic injection of morphia, the perusal of which struck us so forcibly when it first appeared in the pages of *The Medical Times and Gazette.* A child, aged seven, apparently in good health, but known to have suffered from worms, was suddenly seized with formidable epileptiform convulsions, with suffocative paroxysms. The patient was kept alive for some time by artificial respiration. New convulsive attacks supervening, M. Lanceraux proposed the hypodermic injection of the hydrochlorate of morphia. At first seven, and shortly afterwards six, milligrammes of morphia—in all the fifth of a grain—were injected. The good effects of the second dose were almost immediately perceptible, and the child rapidly recovered. On the third day after the attack—which was ascribed to "tickling" of the intestinal tract of the pneumogastric nerve by worms—an anthelmintic dislodged two lumbrici. Although only a single case, we may learn from it an apparently rational treatment for a large class of similar nervous affections caused by reflex cerebro-spinal irritation. In the treatment of infantile convulsions time is everything; and the certainty and celerity with which the antispasmodic and sedative action of morphia is obtained by the hypodermic method of its administration, might, as Cormack shows, be advantageously borne in mind in dealing with these very serious and often rapidly fatal cases.

The articles relating to diphtheria, croup, and other affections described under one or other of these terms, or under convertible, and, consequently, confusing aliases, will well repay perusal—especially when, as at present, renewed attention has been drawn to the subject in anticipation of the expected report of the Committee appointed by the Medico-Chirurgical Society. Personally, Sir Rose Cormack believes that diphtheria is a specific, asthenic, general disease, and that croup is but a symptom common to laryngismus stridulus, laryngitis stridulosa, and laryngo-tracheal diphtheria—in other words, synonymous with stridulous breathing. A great deal of the difference of opinion which has arisen in this country as to the identity or non-identity of croup and diphtheria is due to misconception and confusion of the names given to these affections by French writers. Hence the value of articles such as these, written by a competent authority personally familiar with the views and literature of the profession in both countries. All our readers may not be aware that in France croup means laryngeal or laryngo-tracheal diphtheria.

We regret that we have not space to enter more fully into these

and several other able papers, but from the short *précis* we have given, we trust our readers will be able to form some idea of the attractive nature of these interesting volumes.

WORKS ON MATERIA MEDICA.

Experimental Investigation of the Action of Medicines.   By T. LAUDER BRUNTON, M.D.   Reprinted from *The British Medical Journal.*   London: J. & A. Churchill.   1875.

DR. BRUNTON is well known as an accomplished physiologist and therapeutist, and he has the rare gift of imparting profound knowledge in a very lucid and readable manner.   This small book opens with a chapter giving an excellent survey of the conditions which constitute and which modify the standard of health, and the succeeding chapters discuss the principles of the methods of physiological investigation of the action of drugs, especially in reference to the circulation.   Many interesting and suggestive observations catch the eye, and, at page 49, a useful table is introduced, showing the various causes of alterations in the blood-pressure and pulse-rate—a valuable key to many therapeutical problems.

Royle's Manual of Materia Medica and Therapeutics.   Sixth Edition.   By JOHN HARLEY, M.D.   London: J. & A. Churchill.   1876.   Pp. 840.

THIS favourite and long-established text-book appears in a sixth edition, an interval of nearly eight years separating it from the previous edition.   The thorough revision rendered necessary by this long lapse of time has been well and carefully accomplished by Dr. John Harley, to whose original labours materia medica and therapeutics owe so much.   The modern chemical notation is introduced and freely used, the illustrations are numerous and good, and many valuable therapeutic notes and observations are scattered throughout the book.   At page 416 we learn that "salicin is eliminated by the urine in the form of saligenin, the secretion giving a deep blue colour with perchloride of iron."

We will conclude our notice of this useful manual by quoting from it an expression of opinion which runs counter to the views that are at present most fashionable :—" Those who look for the results of experiments on mutilated animals, in the following pages,

will not find them, for the editor is satisfied that this is not legitimate therapeutical inquiry, and that nothing short of a patient survey of the operation of a drug in the entire body in health, and under the variable influence of disease, can furnish the data upon which we may build a proper theory of its action."

The Book of Prescriptions. By HENRY BEASLEY. Fifth Edition. London: J. & A. Churchill. 1876. Pp. 560.

A FIFTH edition attests the success of the editor in supplying a want by the compilation of a catalogue of more than 3,000 ways and means of administering all sorts of remedies, officinal and non-officinal. The prescriber who is fond of second-hand formulæ will find, under some of the headings, more than twenty modes of administering a particular drug, and, with the help of the Index of Diseases and Remedies, can be at no loss to ring any number of changes in a course of treatment.

Syllabus of Materia Medica. Third Edition. By A. HARVEY, M.D., and A. D. DAVIDSON, M.D. London: H. K. Lewis. 1876. Pp. 51.

THE point of this little syllabus is the tabulation of the articles and preparations in the British Pharmacopœia, according to their relative value, with the view of guiding teachers and students to the more important subjects of study. We imagine that most judicious teachers have long ago adopted this sensible idea in their courses of instruction.

The Druggists' Advertiser. New York.

WE have received several numbers of this new journal, which is now strong enough to venture on a second series in an enlarged form. It contains some interesting odds and ends, and a monthly price list of drugs.

A Practical Treatise on Materia Medica and Therapeutics. By ROBERT S. BARTHOLOW, M.A., M.D.; Professor in the Medical College of Ohio. New York: D. Appleton & Company. 1876. 8vo. Pp. 537.

ALTHOUGH the last on our list of recent works on Materia Medica, this is by no means the least of the volumes in question. By its compilation Dr. Bartholow has conferred a boon on the practitioner

and the student of medicine alike. He is well qualified to write
on the subject, for he filled the Chair of Materia Medica and
Therapeutics in the Medical College of Ohio for many years; he
has conducted many independent investigations into the actions of
remedies, and he has had twenty-two years of clinical experience as
a practitioner of medicine.

The " Schema " he has followed is in some respects a novel one.
The work is divided into three parts—the first specifies and
describes the mode in which medicines are introduced into the
organism; the second treats of the actions and uses of remedial
agents; and the third deals with topical remedies. As a rule,
pharmaceutical, botanical, and chemical details are omitted; while
the therapeutical applications of remedies are, as far as practicable,
based on their physiological actions. The author, in a word,
believes in the existence of a science of therapeutics—not, however,
to the absolute exclusion of empiricism—for, as he well observes,
" well-established empirical facts should not be omitted, even if
they are not explicable by any of the known physiological proper-
ties of the remedies under discussion."

Part II. is naturally the most important and largest division of
the treatise. In it are considered the remedial agents which are
used (1) to promote constructive metamorphosis of tissue, (2) to
promote destructive metamorphosis of tissue, (3) to modify the
functions of the nervous system, and (4) to cause some evacuation
from the body. Under the first heading we find the opening sec-
tion devoted to the consideration of " Aliments "—of course from
a therapeutical point of view. The subject is developed in two
succeeding sections on special plans of diet and alimentation in
diseases respectively. " Water " properly heads the list of " Res-
torative Agents," which include cinchona, iron, phosphorus, &c.
" Agents increasing Waste " include the alkalies, mercury, iodine,
&c. The agents used to modify the functions of the nervous
system are (*a*) excito-motors of the spinal cord and sympathetic
nerve, as electricity, nux-vomica, ergot, belladonna, &c.; (*β*) cere-
bral excitants, or antispasmodics, as camphor, assafœtida, valerian,
&c.; (*γ*) cerebral sedatives, which diminish or suspend the functions
of the brain after a preliminary stage of excitement, as narcotics,
anæsthetics, and some of the so-called antispasmodics; and (*δ*)
motor depressants of the spinal cord and sympathetic, as conium,
physostigma, jaborandi, tobacco, hydrocyanic acid, &c. The
" Evacuants " are considered under the groups, " Emetics, "

" Cathartics," " Anthelmintics," and " Urino-genitals," or remedies which act chiefly on the genito-urinary organs.

Part III., on " Topical Remedies," treats of " Antiseptics," including salicylic acid, of which a very succinct account is given; " Counter-irritants," *i. e.*, rubefacients and epispastics; " Blood-letting;" " Escharotics;" and " Emollients, Demulcents, and Protective Agents."

Such is a very brief analysis of the scope and contents of a really valuable work. In it the reader will always find full and accurate information. The bibliography which is attached to each article further enhances the value of this outcome of modern and scientific therapeutical research.

A Course of Practical Histology. By EDWARD ALBERT SCHÄFER. London: Smith, Elder, & Co. 1877. Pp. 267.

WITH the numerous works on histological methods which have been produced within the last few years, considerable doubt might naturally exist as to whether there were room for such as the present. We have, however, no hesitation in saying that Mr. Schäfer's book is a valuable addition to this branch of histological literature. It is at once practical and readable. It does *not* begin with a treatise on the optics of the microscope—a fact which deserves to be commended—but some twelve pages are devoted to giving practical directions as to how to use the various pieces of apparatus recommended by the author. The course of study is then distributed over twenty-one chapters; each chapter being devoted to a distinct subject. In each subject certain preparations are recommended, the method of preparing and examining them detailed at some length, and a short account of their principal features given. While, as a concise and comprehensive epitome of histological methods, Professor Rutherford's little book is of unrivalled value in the hands of an expert histologist, Mr. Schäfer's account of the course at University College will be found most valuable to the student. Those teachers, also, who endeavour to select matter which will give a general idea of the most important methods, and, at the same time, be assimilable in the very brief period which is devoted to a course of practical histology, will derive much valuable assistance from the book.

Report of the Dublin Hospital Sunday Fund for the Year 1876.
    Dublin: Browne & Nolan. 1877. 8vo. Pp. 39.

THE Council to whom is committed the conduct of this most philan-
thropic movement, are to be congratulated on the measure of
success which has attended their labours. " Hospital Sunday,"
1876, took place on November 12. Collections for the fund were
made in 213 places of worship, being an increase of 18, as compared
with last year. The total amount collected from congregations and
contributed by individuals, in 1876, was £3,873 9s. 3d., being an
increase of £263 15s. 11d. over 1875.

The Council point with special pleasure to the fact that the
average collections from the congregations which joined the move-
ment from its commencement have steadily increased from £19 6s. 1d.
in 1874, to £19 15s. 7¼d. in 1875; and to £20 13s. 8d. in
1876. Twenty-four Churches had collections for the first time in
1876. It will be seen from a table in the report that the Hospital
Sunday movement has not caused a falling-off in the gross receipts
from subscriptions by the participating institutions. On the con-
trary, the voluntary contributions received in 1875 by the eight
hospitals which joined the movement from its commencement were
upwards of £500 in excess of those for 1874.

The Committee of Distribution recommended the distribution of
£3,600 to the participating institutions, and this recommendation
was acted on. The sum distributed in 1874 was £3,000; that
distributed in 1875 was £3,450. The elaborate tables included in
the report contain a fund of information respecting many of our
metropolitan charitable institutions, and are well worthy of being
carefully studied. It should not be forgotton, in conclusion, that
the success which has so happily crowned the " Hospital Sunday "
movement in Dublin is in no small degree due to the untiring
energy and zeal of a respected member of our profession.

Dangers to Health in our own Houses. By T. PRIDGIN TEALE,
    M.A. London: J. & A. Churchill. Leeds: Charles Goodall.
    1877. Pp. 43.

THIS is the substance of a lecture delivered before the Leeds Phi-
losophical and Literary Society, on January 23, 1877, by Mr. T.
Pridgin Teale, Surgeon to the General Infirmary at Leeds. It is
a pamphlet which should be in the hands of all householders, for

its trifling cost—one shilling—is its least recommendation. Mr. Teale's aim in giving this lecture before a non-medical audience was to endeavour to convince his hearers, and, through them, to spread the conviction still further—*firstly*, that it is the duty of every householder to ascertain for himself whether his own house be free or not from well-known dangers to health; *secondly*, that such dangers are real, are at our own doors, and are extensively prevalent.

A most instructive, albeit a popular, discourse, this contribution to the literature of Preventive Medicine is rendered extremely valuable by a series of very graphic diagrams illustrative of sanitary defects which had come under the author's observation in private houses. No one can look at these diagrams and not be impressed with the necessity for domestic sanitary reform. As Mr. Teale submits, in the words of Horace:—

> " Segnius irritant animos demissa per aurem
> Quam quæ sunt oculis subjecta fidelibus, et quæ
> Ipse sibi tradit spectator."

Transactions of the American Neurological Association for 1875. Edited by F. P. KINNICUT, M.D., and T. A. M‘BRIDE, M.D. New York: S. W. Green. 1875. Pp. 257.

THE American Neurological Association has been formed, as its name imports, for the cultivation of neurological science, both in its normal and pathological relations, and its list of members includes the best known names of those who have devoted themselves to this branch of medical science in the United States. The volume before us contains many excellent papers, both theoretical and practical, amongst which we may mention an elaborate essay " On the Structure of the Nervous Tissues and their Mode of Action," by Dr. Schmidt, of New Orleans; "A Contribution to the Study of Myelitis," by Dr. S. G. Webber, Boston; " Pigmentary Deposits in the Brain, of Malarious Origin," by Dr. Hammond; "A Contribution on Insanity in Children, produced by Masturbation," by Dr. Kinnicut, New York; "On Phosphorus in the Treatment of Neuralgia and other Neuroses," by Dr. N. B. Emerson, New York, &c., &c. The book is neatly got up in the style of the West Riding Asylum Reports, and we may fairly congratulate the Association on this the first-fruits of their labours.

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

SAMUEL GORDON, M.D., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, 5th April, 1877.

DR. HENRY KENNEDY in the Chair.

THE adjourned discussion upon Dr. Finny's paper on "Intra-thoracic Tumour simulating Aneurism," was resumed by

DR. MACSWINEY, who said that the rare and difficult case which had been submitted to them, showed that it presented symptoms which rendered diagnosis extremely difficult—symptoms common to either aneurism or intra-thoracic tumour of some other description. The tumour in Dr. Finny's case was, however, both intra-thoracic and extra-thoracic. Having further referred to some of the points which constitute the difficulty of diagnosis in these cases, Dr. MacSwiney asked whether any indication of pressure on the innominate or subclavian arteries existed in Dr. Finny's case, and whether or not the pulsations of the heart and the pulsations of the tumour were identical or isochronous. If the pulsations were isochronous with those of the heart, it would point to the tumour being an aneurism. Then the circumstance of the pupils being unaltered had some force, although in itself not conclusive. He desired to ask did the site of maximal dulness of the tumour correspond with the site of pulsation? In aneurisms the rule was that the sites of maximal dulness and of impulse respectively, were identical. Taking all the reported cases of aneurism as they found them, the presence of the hæmoptysis which occurred in Dr. Finny's case, and which was of the red currant jelly character, pointed more to the existence of a tumour than of an aneurism. It was well known that in cases of aneurism of the arch terrific hæmorrhage, sometimes producing death, occurred, while occasionally there was a hæmoptysis of the character described by Dr. Finny,

consisting of streaks of blood in the expectoration. He was anxious to hear the opinions of the members on the curious symptom of double reduplication of the heart's sounds. It was well known that double reduplication of the heart's sounds was very rare. For his own part he never heard it. During the last month of his hospital attendance he had a case in which the second sound of the heart was reduplicated, the patient, according to his diagnosis, being the subject of mitral stenosis. The patient had a well-marked pre-systolic murmur; and at the base of the heart there was a most marked, distinct, and, for a time, constant and permanent reduplication of the second sound. Dr. Finny had called the sound an "echo," a term which best represented the sound in the case also to which he (Dr. MacSwiney) had alluded. The symptom was of interest in an abstract, rather than a clinical point of view, inasmuch as reduplication of one or even both sounds of the heart was present in some persons who enjoyed perfect health, and was also intermittent in site.

DR. GRIMSHAW said he had to confirm the opinions expressed as to the difficulties of diagnosis in the case in question. He examined the case very carefully with Dr. Finny, and he did not think a single point could be thought of relating to the distinction between aneurism and tumour which was not considered. When he (Dr. Grimshaw) saw the case, he found that every one of those points had been considered by Dr. Finny. For his own part he thought the case one of aneurism. With regard to the expectoration of blood, he had recently brought a case under the notice of the Pathological Society, in which the patient died from thoracic aneurism. In this case hæmoptysis was present, but death did not result from any oozing of blood from the aneurism, but from hæmorrhage into the lung.

The CHAIRMAN said he had seen the case, through the kindness of Dr. Finny, and had come to the conclusion that it was not one of aneurism, but further than that he was not able to go. His reasons were—first, that on feeling the woman's pulse he at once observed it was very quick. Generally speaking, aneurism, whether in the chest or elsewhere, did not entail a rapid pulse. The second reason was that the pulsations were not in keeping with the size of the tumour. The third was the existence of a soufflet, very distinct, and what would be called "musical," at the spine of the left scapula—a sign which he never before met with in aneurism. Even in cases of non-aneurismal tumours of the chest, the existence of a soufflet was exceedingly rare. At that moment a man was in the Hospital for Incurables who had been in the Whitworth. In his case the thyroid body was somewhat enlarged. He (the Chairman) believed he had aneurism. He had all the symptoms of thoracic aneurism. Was it possible for an affection of the thyroid gland to give rise to the symptoms

observed in the case under discussion ? Dr. Head had told him of a case
of a man in whom the middle lobe of the thyroid gland, between the
trachea and the œsophagus, was enlarged, and that its enlargement gave
rise to the symptoms similar to those that had been observed in the
present case. Dr. Collins had also found a case recorded in which the
occurrence of the same symptoms was similarly accounted for.

Dr. Finny, in reply, said that from the first he was confirmed in the
opinion that the case was not one of aneurism. The quick pulse was one
of the most important points against aneurism. He had never met with
very quick pulse in aneurism, except in one case of abdominal aneurism,
in which the affection became a secondary false aneurism, and the symp-
tom was due to the want of blood. Dr. Grimshaw's remarks had, to a
certain extent, answered those of Dr. MacSwiney about hæmoptysis.
Dr. MacSwiney had laid stress on that symptom as pointing to the exist-
ence of a tumour, but the pressure of an aneurism on some portion of the
lung tissue might give rise to it. The pressure, either of an aneurism
or of a tumour on the bronchial vessels, might give rise to the passive
hæmorrhage such as existed in the case in question. There was another
form of hæmoptysis occurring in that variety of aneurism known as
"leaking." The radial pulses were not noted, because there was nothing
remarkable about them. A tumour existing in the region of the manu-
brium would involve the innominate vessels and also the left subclavian
vessels; and the *post mortem* showed a sarcomatous tumour involving
those vessels, but not occluding them. The pulsations over the tumour
were systolic in time, isochronous with those of the heart. The impulse
conveyed by the tumour was observed at the sides, and distensile in
character, but to no very marked degree. It was of great importance
whether or not the pulsation was over the point of greatest dulness ; but
it is well known that an aneurism with various layers of fibrin would
cease to pulsate over the most prominent part, while impulses would still
be felt at the sides of the sac.

Dr. Walter Smith read a paper, entitled, "Notes on Electro-
therapeutics." [It will be found at page 451].

A New Galvanic Battery and Hand Commutator.

Dr. Smith also exhibited a New Galvanic Battery, and a Hand
Commutator. Dr. Smith explained the instruments. Since the revival
of the employment of the voltaic current in therapeutics, it had been a
desideratum to have an instrument which would be at once reliable,
effective, and portable. No doubt a battery cannot go below a certain
size without losing efficiency. Of late years the peroxide batteries had
come into great and increasing use. For seven years he had had in

daily use a Leclanché battery, which is composed of a zinc rod immersed in sal ammoniac solution, and of a porous cell filled with charcoal. More recently a modification of it had been brought out, which he now submitted to the Society. It included twenty cells, and was called the Gaiffe-Clamond battery. It differed from the Leclanché battery in having a charcoal plate cemented through the centre of the cells, serving as a porous diaphragm. It was characterised by great simplicity, the only questionable point about it being its enduring powers. The Leclanché battery would work, with intermittent use, for two or three years, and if the one he showed would work so long it would answer satisfactorily. The electro-motive force of the Gaiffe-Clamond cell is less than that of the Leclanché, and its internal resistance considerably higher. Another instrument which he should be glad to see added to their therapeutical apparatus was the hand commutator, which he showed. It was capable of interrupting the current without changing its direction, as well as commutating. The ordinary commutator was very inconvenient in application, because it would require three hands to work it.

Dr. Finny said he desired to ask Dr. Smith, with respect to the case he had submitted to them, whether the grounds on which he based his opinion were secure on one point. He had alluded to atrophy of the thenar and hypothenar eminences in cases of dropped wrist, and had said that the pressure of the painter's brush on the hand caused atrophy of the muscles. He (Dr. Finny) did not understand how palsy, on the introduction of poison into the blood, could be affected by local applications. In the case adduced the man ought to have had atrophy of the muscles of his feet considerably more than of those of his hands, and the extensors of the hand ought not to have been paralysed so much as the flexors, because the pressure was on the flexor muscles and nerves.

Dr. Reynolds asked how long had Dr. Smith the battery in action? A large battery of this kind had been used with a mixture of peroxide of manganese and ferric oxide, but secondary decomposition occurred and it went out of action. He would like to know for what length of time did Dr. Smith's battery stay in action?

Dr. Smith, in reply, thought that Dr. Finny had taken him up as laying too much stress on pressure in determining the localisation of the poison of lead. The chief cause of the paralysis was the specific action of the poison. Why it limited itself to certain muscles in particular cases was unknown. Pressure might favour its development in certain places rather than in others. He had had under his notice a case of a girl who was employed as the assistant of an invalid lady, who weighed twelve or thirteen stone, and who had constantly to put her hand under

the old lady's axilla for the purpose of lifting her, and the consequence was that she got permanent atrophy of the interossei muscles of the hand. With regard to galvanic batteries for medical purposes it should be borne in mind that they were not used in the continuous manner that batteries employed in physical experiments and telegraphic work were, but had very long periods of rest.

Demonstration of a Ready Method of Clinically Estimating Urea.

DR. EMERSON REYNOLDS exhibited a new form of apparatus for the estimation of urea in urine by means of alkaline hypobromite. He said that, in his opinion, Liebig's method of estimating urea afforded the most accurate results when carefully carried out, but the process is troublesome, and therefore unsuited for rough clinical purposes. On the other hand, the hypobromite process (which is but an improved form of the hypochlorite treatment long ago proposed by Dr. Davy) is undoubtedly that which most rapidly affords the desired results, although the latter have no pretensions to a high degree of accuracy. When strongly alkaline hypobromite of sodium is added to urine, pure nitrogen is evolved; this gas is almost wholly derived from the urea, which is known to yield up its nitrogen by this treatment, hence the volume of gas given off approximately measures the urea contained in the given specimen of urine. Various forms of apparatus have been proposed for collecting and measuring the nitrogen, notably those of Russell and West, Blackly, and more recently of Simpson and O'Keeffe, but all these involve the use of specially graduated measuring and other vessels, which are both expensive and not easily obtained. The process which he (Dr. Reynolds) had arranged affords the desired results very rapidly, while the only measuring apparatus required is a common *two-drachm minum measure*, which every medical man is supposed to have, and the rest of the apparatus can be fitted up at a very small cost (about 1s. 8d.), with materials very easily procurable. Dr. Reynolds made an estimation of urea with his apparatus in a few minutes before the meeting.

[A description of the new ureameter would not be intelligible without the aid of a woodcut, but we expect to give a full description of the apparatus, with an illustration, in a future number of this Journal.— THE EDITORS.]

The Society then adjourned.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-NINTH ANNUAL SESSION.

Saturday, March 10th, 1877.

THOMAS DARBY, F.R.C.S.I., President, in the Chair.

Some Cases of Disease of the Uterus in connexion with the use of Alcoholic Stimulants. By JOSEPH P. DOYLE, L.K. & Q.C.P.I.

MR. PRESIDENT AND GENTEMEN,—I shall not occupy the time of this Society by giving an every-day report of my cases; my object in bringing them forward is to draw the attention of the members to the use of alcoholic stimulants by females of ample means suffering from diseases of the uterus.

The first three cases I shall bring under your notice had what many would call hysterical symptoms, and I may state that I was not sent for to attend them for the uterine affection, nor was I consulted with reference to it until some time after they had recovered from their acute attack.

CASE I.—Married, aged about twenty-eight years, has four children, last four months old, which is bottle fed. The mother is one of those affected females that are fond of admiration, good living, and who are inclined to exaggerate their infirmities in order to receive commiseration.

For some years she has been subject to pain in back, extending down thighs, but since the birth of her last baby it has increased, and she has become weak and nervous, and is suffering from leucorrhœa, menorrhagia, and, within the last few days, from loss of appetite, sleep, and cough.

I was summoned to her suddenly, as she had fainted, but she had come to before my arrival. I found her in bed, the face flushed, the left side of it twitching; the pulse are small and weak, 88; heart's action regular, sounds rather clear; skin moist; extremities feel a little cold; temperature 97°; tongue most flabby and coated; there is a disagreeable taste on mouth, with an inclination to vomit. The least noise makes her start, and she complains of forehead ache.

My usual salutation, for some days, on seeing my patient was—"I cannot eat, and feel very weak; can I take such-and-such wine or some brandy."

On making a vaginal examination, the uterus was enlarged low down, tender on pressure, slightly retroversed and retroflexed; os patulous, with a tenacious plug of mucus adhering to it, on removal of which the part presented a raspberry-like appearance.

CASE II.—Aged thirty; never very strong; when a girl suffered from dysmenorrhœa, which was relieved by taking warm whiskey and water; since her marriage she enjoyed very good health, and has three children, the last is three months old; while carrying it she had a tendency to abort on two occasions, but was delivered at full time of a healthy boy, whom she is not nursing.

Since her confinement she has been suffering from menorrhagia, and has found herself getting weak, although having a good appetite and taking some sherry.

Within the last week she found her strength failing her more, and does not sleep well. When seen by me, she was lying on a sofa after vomiting, and the least movement tended to reproduce it; she was exclaiming, in a weak voice, "I am dying," and referred all her illness to the cardiac and epigastric regions; the face was pale; extremities cold; temperature 98°; heart's action very weak, 92; pulse in wrists scarcely perceptible.

I was informed that in getting up in the morning, feeling weak, she took some sherry, which she repeated three or four times; but not finding it to do her good she had recourse to brandy.

For some days it was very difficult to get her to diminish the quantity of alcohol, which consisted of better than half a bottle of brandy, three or four glasses of sherry, and a little champagne, which was ordered to be substituted for the withdrawal of the others.

She began her potations at five or six in the morning, in order to relieve a feeling of sinking in the epigastric region, and a noise in the ears, which lasted up to ten o'clock, and returned about six in the evening, when it did not last so long.

On each visit I had to examine the heart, in order to satisfy her that it was normal. The bowels were rather obstinate, and had to be relieved on two occasions by enemata of soap and water.

A vaginal examination revealed a granular condition of os and cervix.

CASE III.—Married, aged forty-two, has one child eight years old, has always been subject to delicate tonsils. Came under my care for slight hæmoptysis from apex of right lung, and is very nervous about it, as some of her family died from hæmorrhage. After three weeks treatment, the lung affection being gone, she consulted me with reference to a weakness and severe pain in back, cramps, and a sensation of tightness, and pins-and-needles in abdomen.

There is slight leucorrhœa and severe menorrhagia, and she dates back

the beginning of those symptoms to sitting on damp grass two years since, whilst unwell.

The pain and weakness in back have increased so much lately that she is obliged to recline on the sofa the best part of the day; she is very despondent, and near six o'clock every evening the cramps occur in abdomen, and the only thing she finds relieve them is brandy; the appetite is good; she sleeps well; and the bowels are rather free; her face is full and flushed; there is a timid uncertain look about the eyes, the lids of which are continually winking, and she cannot keep her fingers or head quiet a minute; she sighs frequently, and the least noise, or the sudden appearance of any person in her presence, makes her start and put her hand to the cardiac region; the sounds of the heart are clear and the action weak; pulse at wrists very small and compressible, 112; temperature, 97°.

On a vaginal examination the os and cervix were found to be granular.

CASE IV.—Aged fifty. Was knocked out of bed to see her for supposed hæmorrhage; she was under the influence of alcohol; at the request of her friends I saw her the following day, when she apologised for disturbing me, as she was tipsy. She informed me that about two months ago she had a uterine tumour removed, in consequence of metrorrhagia, that produced great weakness, and obliged her to take stimulants in large quantities.

I have not seen or heard from her since.

CASE V.—Aged forty-six; married twice; has one child by last husband, seven years old. For some years is in the habit of imbibing freely; has been suffering from metrorrhagia, that was so severe on one occasion that I had to apply tr. fer. perchloridi to interior of uterus, and plug.

Under the administration of saline purgatives, the uterine congestion was relieved, and she died in a year afterwards of chronic alcoholism. The metrorrhagia depended on enlargement of the liver.

None of my cases had any important hysterical symptoms, such as clavus or globus hystericus, convulsions, spinal tenderness, or articular derangement. The temperature, when taken in the axilla, was generally under 98° F., and in Cases I., II., and III., the abdomen emitted a hollow sound on percussion.

In four of the cases I brought before you, stimulants were first taken to relieve weakness or nervous exhaustion, and which I have no doubt they did.

The late Dr. Gregory states that an occasional excess is, upon the whole, less injurious to the constitution than the practice of daily taking a moderate quantity of alcoholic liquor. The action of alcohol being transient (and, in such cases as I have narrated, increasing the uterine

affection and depression), soon calls for a repetition, and gradual increase in the quantity, so that the system becomes imperceptibly tolerant of a large supply, and which ended by producing an attack of subacute gastritis in Cases I. and II., hæmoptysis in Case III., and intoxication in Case IV.

The great point in connexion with these cases that I observed, was the gradual diminution in the quantity and final disuse of stimulants, which seemed to keep pace with the improvement in the uterine disease.

It is now some months since the three first cases were under my observation, and when seen by me a short time since they were quite well, and I understand only take a glass of sherry occasionally.

The great factor in the treatment of such cases is rest, and those organs functionally deranged generally require it first, but the uterine malady should be treated as soon as possible, for as long as it exists nervous irritation and exhaustion are kept up, and the circulatory system is drained by the menorrhagia and leucorrhœa. I need not say that you must impress on your patient the necessity of giving up stimulants.

The local application I found of most service, after any congestion was relieved by a scarification, is the glycerine of tannic acid.

Before concluding my paper, I may state that I do not disapprove of the use of alcoholic beverages when taken in moderation or applied medicinally. At the same time, I avoid prescribing them as much as possible, and, when needed, I order half drachm doses of spt. vin., combined with tr. of digitalis or spt. ammoniæ aromat.

The PRESIDENT.—This is one of the most important subjects that could be brought before the Society, and I hope gentlemen will express their opinions on it. The question is, whether uterine disease is a cause of women having recourse to undue quantities of alcoholic stimulants, or is it the taking by them of alcoholic stimulants in undue quantities that causes the disease.

DR. DENHAM.—The important question is whether imbibing drink is a cause of these complaints, or whether the pre-existing complaints cause the inclination to use stimulants. It is a question that will come very largely before the profession before many years elapse. I frequently hear the opinion expressed that doctors are too much in the habit of ordering stimulants, and that many patients attribute their intemperate habits to the advice of medical men who had been their attendants at former periods. My decided opinion is that as a rule the disease and tendency to take these stimulants existed before the doctor perhaps ever saw the patient, and that the palpitations, and faintings, weaknesses, and other such symptoms are really put on and assumed by the patients for the very purpose of inducing the medical attendant to

recommend or permit the taking of a certain amount of stimulants. I therefore look on the subject as one in which our reputation is more or less involved. I have no doubt that in many instances of uterine affection the nervous system is engaged, and that a considerable amount of mental and bodily depression and also nervous depression is induced by the existence of the affection of the uterus. It therefore behoves us to be very gentle in our condemnation of patients who seek for stimulants under the circumstances. But I am very strongly impressed with the feeling that they are neither necessary nor advantageous. I believe that tonics, light and nutritious diet, change of air, society, and other constitutional and local means of treatment are amply effectual in such cases; and that the ordering of stimulants to the extent that some patients say they are recommended is unwise and unsafe. I have no doubt that Dr. Doyle's treatment was very judicious. He had, if I remember right, three or four recoveries, and only one death. The subject is one which we shall have to face boldly and at the same time judiciously; and I think we are deeply indebted to Dr. Doyle for bringing it under our notice.

Dr. MacSwiney.—I should greatly regret if Dr. Doyle's paper were suffered to pass without further observations, and I therefore proceed to make a few desultory remarks respecting the communication he has made to the Society. Dr. Doyle, in the cases which he has so well and faithfully related, appears to have met with instances of uterine disease in which the patients were addicted to the use of excessive quantities of alcoholic stimulants; and he has implied, I think, that he is rather of opinion that those stimulants relieved the symptoms, and therefore were not to be countermanded—at least I understood him to imply as much. Now two questions, I conceive, arise in connexion with the paper. One is—is a tendency to the abuse of alcoholic stimulants characteristic, in a special manner, of uterine disease; and the other is—is the use of alcohol to be recommended in the treatment of various troublesome and distressing symptoms that females suffer from who are affected with uterine maladies. Reflecting upon the cases that from time to time for a number of years have come under my notice, I am surprised, in the rapid retrospect which at this moment I make, at the number of cases of disease of the uterus which I have met, in which the patients were disposed to take more wine and brandy than could be conscientiously recommended by me. Dr. Denham, as I understood him, has thrown out, as the result of what we must know to be very extended experience, that perhaps in these cases there is no connexion of cause and effect between the uterine disease and the tendency to imbibe alcohol, but that they co-existed in the same patients. For myself I will say that, whilst I recognise that the use of alcohol most unquestionably has a temporary effect in alleviating the

distresses of the uterus as well as of other affections where the nervous
system is a good deal affected, I am disposed to regard the concurrence
of the organic affection and the alcoholic tendency as only an accidental
coincidence. Dr. Denham has heard medical men charged with pre-
scribing wine too freely for their patients, and thus originating the bad
habit. Certainly all, or most, medical practitioners are obliged to prescribe
stimulants sometimes; but my experience—and I have considered the
question carefully—is not in accord with that of those persons who
prefer this accusation. I can confidently say that the great body of
medical men are not open to the charge of prescribing alcohol for their
patients unduly, or unnecessarily, or thoughtlessly; and I have not come
to that opinion without having endeavoured to ascertain precisely what
the facts were. I am aware, as many other members of the Society
must also be, that the blame is constantly laid on the shoulders of the
doctor by many persons, particularly females who have already acquired
habits of intemperance before consulting a doctor. They will often
state untruly, as I have personal knowledge, that these stimulants were
first prescribed for them by the physician; and thus it has become the
fashion in society now, as many members must know, to make the charge
against medical men that they do great mischief to the community by
the thoughtless and free manner in which they inculcate the use of
alcohol. I believe that to be a calumny, and I think it is to be regretted
that any sanction should ever be given to it by any act of members of the
profession. With respect to the opinion that uterine diseases may be
treated with safety and benefit by moderate doses of alcohol, I am sure
occasions do arise in which alcohol may be thus taken; but I cannot
subscribe, from my own experience, to the opinion which the learned
reporter of the case seems, if I caught his words correctly, to hold, as to
wine being a good remedy for uterine affections. On the contrary, I
have found that the greatest temperance, and the greatest avoidance of
stimulants, using the term in the widest sense, and meaning thereby not
only alcohol but various stimulating articles of food and condiments, is most
beneficial in the treatment of these cases; and that on the other hand the
free use of these stimulants has exercised an injurious effect on the vast
majority of uterine affections. And I hold that in the cases of other-
wise healthy young females who may be suffering from a congested
condition of the body and cervix of the uterus, together with, perhaps,
some endo-cervicitis, and abrasions of the os uteri, the exhibition of wine
is to be for the most part discountenanced and avoided.

Dr. HENRY KENNEDY.—The meeting will take up an erroneous idea
if they look on this subject as a specific one. Everyone familiar with
the practice in such cases cannot but be aware that many cases occur
exactly similar to those which Dr. Doyle has mentioned, with the excep-

tion that the uterus is not concerned in the matter at all. We all know that in diseases of the stomach there are numerous cases in which the patients are constantly making excuses for taking stimulants, and asking their physicians in reference to the use of them. These are exactly analogous cases to Dr. Doyle's, and I do not believe that the uterus has a pin's worth to say to them. We shall make a great mistake if we limit our observations on this question to females. 1 believe that men are just as subject in their turn to affections arising from the excessive use of stimulants as females; and I should be very slow in saying that there is any necessary connexion between diseases of the uterus and the necessity for stimulants. I believe it would be a great mistake to look on the matter in that point of view. It is a great general question, which it would be worthy of a Society like this to face, although it involves a great many points which we are delicate in speaking on. When a physician advises that a certain quantity of stimulants should be taken, it should be clearly understood that the patient is strictly to adhere to his advice as regards the prescribed quantity; whereas we know that, as a matter of fact, he oftendoes not do so, but goes a great deal further than his physician advises. No physician at the present day will order more stimulants than he believes in his heart to be useful; but that the stimulants so ordered by him are useful I have not the slightest doubt. If we do not use stimulants in such cases we will be driven to use their equivalents. If a man with an ulcer in his leg has a languid circulation, you must either give him a little wine or order some powerful tonic—strychnine, ammonia, or something else that will supply the place of the alcoholic stimulant. I should be very slow in saying that stimulants must not be used in a great many cases. There are a vast variety of cases that would not go on at all if you did not give the patients stimulants; in fact the question of stimulants is not a question merely of strong drink at all but a question of life and living. All life involves stimulus. The very air we breathe, the food we use, and the exercise we take, are stimuli more or less. But if a patient uses more stimulants than his physician orders, the latter is not to be blamed. We must not loose sight of the great fact that in one shape or another— whether externally in the form of caustic, or internally in that of brandy—we must occasionally use stimulants. I believe that medicine would not exist at all if we were to say that we should give up the use of stimulants; and no sufficient reason for our saying so is afforded by the fact that unfortunately in many cases the use of stimulants is carried to excess by the patients themselves. But I deny that the physician or the surgeon either ever prescribes them in such quantities as to be injurious. Our knowledge of physiology at the present day is quite sufficient to maintain us in our position that we must use stimulants, no matter what may be their character or degree, whether as physic or as

strong drink. I repeat that the question which Dr. Doyle has brought before us is a great one; and that although it is a delicate one, and in connexion with which the public blame us, we are in a position to maintain that the treatment of disease will not allow us to go on without stimulants.

The PRESIDENT.—The cases related by Dr. Doyle bear not so much upon the question of alcoholism as upon whether certain diseases of the womb which he has met with are connected with habits of intemperance. The great question is, whether the diseases of the womb merely coincide with intemperance, or whether the presence of the diseases induces debilitated patients to indulge to excess in alcoholic stimulants. With regard to the moral question of whether medical men are to blame or not in ordering stimulants, I think we have a right to assume that they order what they think necessary for the patient; and if the patient chooses to poison himself with alcohol, I do not think the doctor is responsible. If the patient abuses the advice of the doctor that is his affair. I am quite aware that the opinion of certain classes of the public at the present day is, that medical men are to blame in this matter. I do not feel that I am the least to blame if I order brandy or wine for a patient, for I order it because I think he wants it, and if he chooses to take a great deal more than I order for him, I do not hold myself at all responsible for it. That is the proper way to look at it—*fiat justitia ruat cœlum.* The great point raised by Dr. Doyle's paper is the question of cause and effect—are the diseases induced by alcohol, or do they induce the tendency to take it; and I think it is very desirable to discuss that question in a philosophic spirit.

Dr. M'CLINTOCK.—As I did not hear the paper which has led to the present discussion I cannot venture to say much. The chairman has very rightly called us back to what is the real subject for our consideration—namely, the connexion between uterine diseases and intemperance. The general question as to the use of alcohol is no doubt one of vast importance in a political, social, medical, and physiological point of view, but it is rather foreign to our present purpose. Confining myself, then, to the real point of Dr. Doyle's paper, I may observe that this question of the connexion between uterine disease and the improper use of alcoholic stimulants—a question, I may observe, of great interest— has not hitherto been brought before this Society. There is a great deal of truth in the observations that fell from Dr. Denham. I am inclined to think that uterine disease is more frequently the cause of intemperate habits than the effect of them. In most cases of chronic uterine disease we know that there is a state of cachexia—a condition recognised by some of the older physicians, and appropriately designated

by Hoffman under the name *Cachexia uterina*—which very truly expresses a condition of things that we are constantly meeting with. It is attended with constitutional debility, langour, and apathy. In a large majority of the cases of endometritis and leucorrhœa this is the case. The patients lose all energy. Everything that they have to do involves effort, and seems to them a labour and a toil; they have no elasticity, and take no pleasure in going about their ordinary household affairs, and, consequently, they resort to the use of stimulants of one kind or another. I have from time to time seen several cases of extreme intemperance in women, five or six of which went on to a fatal issue, in consequence of the unlimited employment of stimulants of one kind or another; and yet, in these five cases which ended fatally, not one of the patients had any uterine disease. At the same time, I do not for a moment pretend to say that the excessive use of stimulants may not induce uterine disease. It induces disease of the stomach and liver, and there is apt to be congestion of the abdominal viscera, which sooner or later involves the uterus. I have seen several cases of metrorrhagia and menorrhagia, which apparently resulted from the excessive use of alcoholic spirits.

Dr. KIDD.—Having been detained elsewhere, I unfortunately did not arrive in time to hear Dr. Doyle's paper. I think it probable, however, that I saw one of the cases mentioned by Dr. Doyle along with him—at least I saw one case with him in which there was a very considerable amount of uterine disease, and in which the patient had given way to habits of extreme intemperance. Dr. Doyle has detailed the case more accurately than my recollection enables me to do; but the uterus was very much congested, there was a considerable amount of hæmorrhage going on both at the menstrual periods, and sometimes during the intervals between them, and there was also a leucorrhœal discharge. The uterus was large, the os was very open, and there were all the evidences of congestion of the whole organ. The liver was enlarged also; and the woman was bloated and had a livid countenance. She had constant vomiting and nausea, and Dr. Doyle and I were both of opinion that the uterine disease was the result of habits of intemperance, in the same way that disease of the stomach and enlargement of the liver very often result from them. That habits of intemperance will in this way give rise to uterine disease I have not the smallest doubt. My experience quite confirms what Dr. M'Clintock has stated—that in many instances where you have habits of intemperance you will see uterine disease of that character in connexion with them. My experience also confirms the remark of Dr. Denham, that uterine disease often gives rise to the desire for stimulants. I am afraid that Dr. MacSwiney has strangely mis·apprehended Dr. Denham's remarks. If I understood him rightly, Dr. Denham admitted that this uterine cachexia which Dr. M'Clintock has

spoken of will give rise to habits of intemperance; and really we cannot wonder at it, for there is no doubt that in many cases of uterine disease stimulants afford a temporary relief that nothing else will. In the very common disease of dysmenorrhœa, I believe the universal practice among old women is to administer to the patient, in England, gin, and in this country, whiskey or brandy-punch; and I know that in the majority of cases nothing will give such prompt relief as a good large dose of stimulant of that kind; that, I am sure, we all meet with every day. Now, consider the effect of a young woman being taught, as she enters life, that every month or every three weeks she is to go to bed and have a good large half-glass or more of whiskey or brandy given to her; and if they take brandy, they sometimes say it will not do them good unless they take it raw. A young lady told me once that a sip did not do, but that she should have a good large mouthful and should feel it go down. The relief thus afforded is almost instantaneous; they sleep off the effect of the stimulus, and get up freed from pain. In cases of endometritis, where you have constant pain in the back and constant discharges, and perhaps every month hæmorrhage more or less going on, you have cachexia as the result. Now in that case the desire for stimulants is constant; and the relief obtained from the stimulant, which lasts some hours, induces the patient to fly to the same remedy very frequently, resorting to it again after the effect of the dose has gone off. I believe with Dr. Denham and Dr. M'Clintock that it is this which so often gives rise to habits of intemperance amongst women, and that in the majority of cases, instead of the habit of intemperance being the cause of the uterine disease, it is the uterine disease which is the cause of the habit of intemperance. The practical lesson to be learned from this is, that we must strive to cure the disease, so as thereby to do away with the cause of the desire for stimulants. That is the true way to check intemperance amongst women, and not by mere restrictions of the use of stimulants. Alcohol is not the only stimulant that patients of the class under discussion have recourse to. Some derive great relief from morphia, and frequently use it. One of the heads of one of the largest of our medical establishments came to me some time ago and asked was I aware of the extent to which a lady, a patient of mine for whom he was in the habit of making up my prescriptions, was using morphia. I had never ordered morphia for her; but she had been previously taught the use of it, and she was in the habit of going to this establishment and ordering from six to twelve dozen half-grain doses. That taught me the lesson to be very careful about ordering morphia.

DR. JOHNSTON.—I can corroborate what has been said as to the state of the stomach being connected with disease of the uterus. During my term as Master of the Lying-in Hospital we had a number of cases of

uterine ulceration, which were invariably attended with dyspepsia; and for the swelling of the stomach after food a little peppermint or some other stimulant was generally taken. It is in that way that I think poor creatures often contract the habit of using, and then abusing, stimulants. As to dysmenorrhœa, the common remedy for it is a tumbler of brandy or whiskey-punch.

DR. DOYLE, in reply.—As to the cause of the habit in the cases I have brought forward, the patients were not to blame in my opinion. The disease, as far as I saw of them, was the cause of their intemperate habits. Dr. MacSwiney altogether misunderstood me, when he said he thought I approved of it. As I have stated in my paper, the habit of drinking is brought on in this way. The patient first gets weak from leucorrhœa or menorrhagia and uterine exhaustion, and is induced to take a glass of wine; and this practice being repeated, the habit of drinking is imperceptibly formed. In Cases I. and II., mentioned in my paper, I was first called in to prescribe for acute gastritis; and in Case III. for hæmoptysis. Case V. was the one of chronic alcoholism and disease of the liver that Dr. Kidd alluded to. When I cured the disease of the uterus, the tendency to take alcohol disappeared; and, I am happy to say, with respect to those three cases in which there was a granular condition of the uterus and inflammation of the interior of the womb, that all are quite well at present, and do not take alcohol or anything beyond a glass of sherry occasionally. Dr. Kidd alluded to the relief afforded to dysmenorrhœa. I mentioned that one of my patients, when a girl, took whiskey-punch to relieve dysmenorrhœa; and when she got married she had no occasion to take it. After the birth of her first child, she got weak, and the uterus became slightly enlarged and she suffered from menorrhagia and leucorrhœa, and began again to take stimulants.

Note of Case of Diphtheria. By R. J. KINKEAD, M.D.; Professor of Obstetric Medicine, Queen's College, Galway.

A. G., female, aged about seven years, had been ill for some days before I visited her. When I first saw her, she presented the following symptoms:—

Respiration slightly hurried; larynx evidently obstructed; a hoarse sawing sound was produced by both inspiration and expiration; voice low and hoarse; cough, croupy, barky, and metallic; temperature 101°; pulse 130; tongue white; bowels lose; urine scanty and high-coloured; face flushed, but no appearance of diminished aeration of the blood; throat red, but no appearance of false membrane; no glandular enlargement on outside. Date of visit, 8 p.m. on 23rd inst. Diagnosis, croup. I saw her twice a day on the 24th, 25th, and 26th, when the symptoms had so much improved that I only visited her once a day afterwards. The

temperature fell to the normal; the bowels became regular; the pulse fell to 90; the stridulous breathing disappeared, and became even, noiseless, and about the normal rate; the tongue cleaned. Improvement continued till the 30th, when the tongue got a little dirty.

On the 1st February the throat was covered with a false membrane. Marked asthenia had set in; the child was listless and heavy; breathing perfectly easy; pulse 130, very feeble; temperature below normal standard. During the night of 1st February convulsions occurred, and she died at 11 30 on the 2nd.

On the 30th, difficulty of swallowing manifested itself, the food (liquid) regurgitating through the nose. A day or two before this the food irritated the larynx, causing coughing. There was no appearance of false membrane till the 30th. I was, unfortunately, prevented from visiting her on the 31st, and the great change which had come over her between my visit on the night of the 30th, and the 1st of February, was most startling. The appearance of false membrane, on the 30th, was very slight; so much so that, in the very imperfect light, I was not able to decide whether it was the beginning of a pellicle or mucus from the posterior nares. The improvement of the patient from my visit of the 29th was, however, so marked, that I attributed it to the latter cause. She appeared in every way better, and there had been a decided improvement in appetite. The treatment was, in the first stage, when there was every symptom of croup, half grain doses of calomel with vin. ipecac. and syr. scillæ—the latter to be given whenever the breathing became much obstructed; this produced marked benefit, and so steady was the amendment that only four doses of the mixture were given; and, as the child improved, the intervals of the calomel were increased from every hour on the first night, to every third, fourth, and finally ceased on the 27th. On the 26th, she was put on chlorate of potash and bark; and, on the 1st February, carbonate of ammonia was added.

On the 24th, a blister was applied to the trachea over larynx, and linseed poultices, changed every third hour, were kept to the throat all through; the blistered surface had perfectly healed on the 28th. Diet, at first, milk, *ad libitum.* On the 25th, beef-tea was added. On the 27th, port wine two ounces, with yolk of egg. Another ounce was added on the 30th, and it was ordered to be given freely on 1st February. The throat was painted with liquor ferri pernit.

The question appears to be—were the croup and diphtheria distinct affections, or was it one and the same disease which attacked first the larynx and then spread to the pharynx.

The diphtheritic membrane did not spread upwards from the trachea, but appeared to commence on the upper border of the pharynx. The marked improvement of the patient, and the apparent recovery from the croup, no symptom of laryngeal disease save aphonia re-

maining, seem to indicate that the diphtheria was a second independent disease.

I have not been able to trace any contagious element. There is no water-closet, nor any communication with the sewers from the house. The slops, I was assured, were emptied at a distance from the house.

On the other hand, there was one case of diphtheria in the same neighbourhood, which developed itself in a gentleman, the very day he arrived in Dublin from Tuam, where he had been staying. Also scarlatina has been prevalent in the town and country. And in two cases I attended, the stage of convalescence was complicated by croup. It is curious that in both these cases there was, apparently, contagion—the elder boy, aged about three and a half, had scarlatina rash out when I first saw him, and his brother was then in the room; about six days afterwards, the sick boy being isolated, the brother got a very mild attack, and was then put into the same room. When the first boy had nearly passed through the desquamative process, he got an exceedingly bad attack of croup; and as he got well, his brother got a mild attack. In neither could I detect the least trace of false membrane in the pharynx.

The weather has been exceedingly trying; a vast amount of wet, with cold, stormy wind; whilst the county about is tremendously flooded.

The CHAIRMAN (Dr. MacSwiney), said.—Dr. Henry Kennedy, in a former paper, raised the question as to the identity or non-identity of croup and diphtheria, and it will be recollected that he used strong arguments to support the opinion that they were totally distinct affections. You will perceive from Dr. Kinkead's paper that his attention was directed to the question by Dr. Henry Kennedy's paper, and it is now for you to consider whether the case he has sent supports Dr. Kennedy's paper, or shows the identity of the two affections.

DR. HENRY KENNEDY.—I do not think the case submitted to us by Dr. Kinkead tells one way or the other. A girl, aged seven years, gets an attack of hoarseness, with hard croupy cough, and, after a day or two, diphtheria makes its appearance. I think that different diseases may attack the same part. There may be diphtheria in the larynx as well as croup. Dr. Kinkead describes the child's throat as quite red, although there was no pellicle. That is a state of things which is not seen in croup. That is a very important distinction, and it rests not on my observation alone, but is supported by the very large experience both of Dr. Cheyne, the elder, and his son. They expressly state that they saw nothing wrong with the internal fauces in the cases which they report. On the other hand, the first time Dr. Kinkead sees this child, he finds the throat red. I take it that the case was wholly one of diphtheria. At the end of four or five days the pellicle of diphtheria makes its

appearance, and the child dies, as they frequently do in such cases, with convulsions. I repeat that from first to last there was no croup in the case. The disease was not confined to the larynx; and the fever was of that inflammatory character which accompanies diphtheria. On the whole, I repeat, that the case appears to me to be one of diphtheria complicating the larynx. It would be out of place now to enter into the differences that appear to me to exist between the two diseases.

A DISCOVERY IN PHYSICAL DIAGNOSIS.

DR. EDGAR HOLDEN describes (*N. Y. Med. Record,* Jan. 20) a plan for facilitating the early detection of phthisis. A soft rubber tube $\frac{1}{2}$ of an inch in internal diameter and 2 feet long, with simple end-pieces of thin metal, and of a diameter of $\frac{3}{4}$-inch, will, when blown into with a little force, produce a rushing noise at its extremity. The same sound is produced by forced inspiration. This is the instrument required. If the patient be made to respire through this, the ear of the physician being applied to the chest, and particularly at the supra-scapular space, this rushing sound is transmitted with clear resonant volume. Disease, however slight, exaggerates the sound, alters the pitch, or changes it in proportion to the solidity of the conducting tissues. Obliging the patient to hold the extremity of the tube away from the listener prevents distracting his attention from the transmitted to the real sound, as does also the closing of the free ear with the hand. A singularly magnified character is given to the respiratory murmurs, and the stethoscope is unnecessary. In thin persons, so great is the exaggeration of the natural sounds that, as with the stethoscope, comparison of the two sides may at times be requisite to prevent misinterpretation; but in local consolidations and small cavities it has proven invaluable. Dr. Holden presents a few cases in which this device—for want of a better name termed a "resonator"—has proved useful. He summarises his observations by claiming that it intensifies the sounds of vesicular dilatation, whether in a normal or morbid state. It intensifies the tubular sounds which, to the unassisted ear, are sometimes partially drowned by the neighbouring healthy murmurs, and it exaggerates to painful hoarseness the evidences of air in cavities. Further experience will undoubtedly define more clearly the amount of assistance it will render in a disease which is manageable only in proportion to the promptness of its early detection.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.

President—THOMAS HAYDEN, F.K.Q.C.P.

Secretary—E. H. BENNETT, M.D.

Extra-uterine Fibrous Tumours.—DR. F. T. PORTER said: This specimen was removed from the body of a female subject in the Ledwich School of Medicine. She was apparently about forty years of age, and appeared to be well nourished. In fact, I could detect no cause of a nature likely to account for death. The specimen is interesting, on account of the great number of these fibrous tumours. They appear attached by a pedicle to the fundus of the uterus. The os uteri is in the condition which is generally called " pin-holed," and is also thickened and everted. The substance of the ovaries is healthy; but attached to the right ovary is a small cyst. I, yesterday, showed a section of the specimen at the College of Surgeons, to Dr. Barker, who, without a microscopic examination, but merely from an inspection, pronounced it to be fibrous. So far as the woman's general condition and organs were concerned, she was healthy; and the specimen is also interesting on account of its size. The existence of such tumours may be accounted for by the hypothesis that the " pin-holed " os caused mechanical dysmenorrhœa, which, owing to menstrual obstruction, caused the products of menstruation to evolve such growths as the tumours in question, which were in various stages of development, from the size of a gooseberry to that of an orange.—*December* 16, 1876.

Myxo-sarcoma of Breast.—DR. J. W. MOORE said: Through the kindness of my colleague, Mr. Porter, I have the opportunity of laying before the Society a remarkable specimen of myxomatous sarcoma of the breast. Last Monday I saw this tumour removed by Mr. Porter, in the theatre of the Meath Hospital, from the left breast of a woman, aged thirty-five years. The clinical history of her case, accurately taken by Mr. J. A. MacCullagh, Resident Pupil, is briefly as follows:—When seven months gone with her first child (six years ago last August), she noticed her left breast getting black and very hard, with a good deal of pain now and then in it. After her confinement she nursed the child for only seven weeks, fearing she might do it an injury. There was a great deal more milk in the sore breast than in the other. Her breast remained hard, black, and sometimes painful, until her second child was born early in July, 1871. She nursed this child for four months, and then, five years ago last Hallow Eve, her breast broke out, and she nearly bled to death. Immediately after this breaking out, the breast began to

grow, and has been growing every day since; there was a running from
it for three months, and one or two small abscesses formed under her arm.
These very soon healed, and the running stopped. Every month since
her second confinement, when her changes, which are quite regular, were
on, there has been some bleeding from her breast, but never to signify,
until last November, when she lost a good deal of blood. Since then,
blood and some stuff like jelly have been running from it. She has never
slept with her husband since her second confinement, as her breast
troubled her too much, being always rather more painful when her
changes were on. But for this she has always been healthy and strong.
She never got any hurt in her breast, but stated that she used to wear
very tight stays up to her first confinement. All her people were very
healthy. Her first child, a girl, is alive and well; her second, a boy, died
when nine months old. The tumour, of course, did not, by itself, weigh
5 lbs. 4½ ozs., but this was the weight of the whole mass removed in the
operation. The tumour presents a very peculiar appearance. In the
centre of it we come upon a large cavity in which a separate tumour lies
embedded. A microscopical examination of this internal tumour was
made by Drs. Robert M'Donnell and Reuben J. Harvey, who report that:—
" It consists of somewhat small spindle-cells, with a small and ill-defined
intercellular substance. On tearing a piece of the tumour in two, it pre-
sents the stringy appearance met with in mucous tissue. The amount of
mucin present is but small. Some of the dark portions, supposed, on
macroscopical investigation, to be blood clots, show essentially the same
structure. Their dark colour is due to blood extravasated into the
tissue. A portion of the embedded mass floats in Müller's fluid, and, on
examination, is found to consist almost entirely of pure fat-tissue. There
is, however, no sharp line of demarcation between the lipoma and the
surrounding tissue, and the spindle-cells are found passing in all directions
between the fat-cells." Drs. M'Donnell and Harvey regard the tumour
as an example of myxo-sarcoma, and they are of opinion that the lipo-
matous portion was an after-development from the sarcomatous elements.
Of the tumour in general, Dr. M'Donnell has been good enough to make
the following detailed report:—" This tumour is a sarcoma, and a sarcoma
of that kind which pathological histologists now-a-days recognise as
' myxoma.' In other words, it is a tumour of the connective tissue series,
and resembling in its structure that variety of connective tissue met with
in the funis, and named ' mucous tissue.' Histologically, it is composed
of cells of different forms—round, oval, fusiform, or stellate. These cells
have, in no respect, the character of epithelium, they do not belong to the
epithelial group of cell-growths. In this particular tumour the oval and
fusiform cells predominate. Chemically, mucous tissue is characterised
by the presence of mucin—this gives a gummy mucilaginous character to
the juice of this tissue; it draws out into threads—'*filant,*' as the French

call it. Mucin coagulates, on the addition of acetic acid, but is not re-dissolved by excess of this acid. This tumour contains mucin, but by no means in quantity so great as these tumours sometimes do, I should add that these tumours, as a rule, do not affect the lymphatic glands."

In this case there were no enlarged glands. But they are apt to return *in situ;* and the probability is that, unless every particle of this growth has been removed, it will return either in the neighbourhood of the breast, or in tissues near some internal viscera, such as the liver or the kidneys.—*December* 16, 1876.

Impacted Fracture of the Shaft of the Femur; Separation (compound) of the Superior Humeral Epiphysis.—DR. BENNETT said: These specimens are fractures, the result of a tram car injury. There was nothing special in the mode of their occurrence except its violence. A boy, aged about fifteen or sixteen years, large and powerfully built, running across Brunswick-street in the dark, and, I dare say too, a little confused by having taken a quantity of porter, was struck by the pole of a tram car, which was passing at a trot. The collision had occurred without any effort to check the vehicle, and he was, of course, struck down by both the pole and the horses. Further than this we do not know how the blows were inflicted. He was brought to Sir P. Dun's Hospital, and it was manifest on his admission that he had sustained a fracture of the thigh. In the removal of his clothes, as he lay on a bed, some difficulty was experienced in taking off his jacket, which was found to result from the fact that the humerus had been driven through the skin on the right side, and that it had also gone through some part of his clothes. In addition to these injuries he bled freely from the ears. He was perfectly conscious. He suffered from collapse, but could both hear and see distinctly, and answer questions rationally. He died in about four or five hours after his admission, without hæmorrhage, further than that which had occurred in the neighbourhood of the fracture of the thigh and from the ears, which was not to such a degree as by itself to be fatal. He died before reaction took place by failure of respiration. In spite of the fact that he was perfectly free from deafness, and perfectly conscious, we found on *post mortem* examination that he had an extensive fracture of the base of the skull, which traversed both the petrous bones and the proclivus. The fracture started from the squamous portion of the left temporal, passed through the meatus on that side, through the petrous portion, and across the base through the proclivus, and so to the opposite petrous. I mention these facts as they prove that a fracture of the skull may even involve both the ears without producing any loss of hearing. The fracture of the thigh presents some points of interest. As is the case in most fractures inflicted with such violence it is comminuted, and involves the greater part of the lower third or even half of the shaft. The first

point of interest is the enormous extent to which the periosteum has been separated from all the upper fragments. This is done so cleanly that it is quite evident that it was not the result of any dissection during the separation of the specimen from the body. Again, from the lower part of the bone, we see a similar stripping to even a greater extent, and more than one would · have been inclined to suppose would have been likely to occur in the case of a comminuted fracture without perforation of the skin. The next point of interest is, that while the primary fracture is comminuted, there is a large piece of the femur belonging to the upper fragment impacted into the lower third of the bone. It is a piece of considerable size, for, although its position conceals it from full view, we can exactly measure its dimensions by the defect in the posterior surface of the bone when the fragments are arranged in their original positions—the measurements are 2⅓ inches in length by an inch at the greatest breadth. The entire of this piece at first was missing, and we did not discover that it was impacted.

Failing, on careful examination, to find it among the lacerated muscles of the thigh, we were led to examine the fragments minutely; we discovered it buried as it still is in the medullary cavity of the lower fragment. It has been driven in edgewise into the lower fragment, and has burst the compact tissue, so that the fissure which runs towards the articular extremity is held gaping widely, and, close to the condyle, the lowest point of the fragment has burst the compact tissue open afresh, and has almost escaped through it. The bearing of this impacted fragment to the lower extremity of the femur shows that the impaction was effected after rotation of the knee outward had occurred—a fact noticeable in the majority of impacted fractures of the neck of the femur. The impacted piece is evidently the apex of the upper fragment which was at first sharply pointed, in consequence of the extreme obliquity of the primary fracture.

The fracture of the right humerus is interesting, as it proves to be a separation of the superior epiphysis, with comminution of the diaphysis close to the line of the epiphysary cartilage. When the boy was admitted and his clothes removed, the diaphysis was seen protruding through the skin on the outside of the limb above the groove of insertion of the deltoid muscle. The efforts made to reduce it previous to my arrival at the hospital had not succeeded. Full two inches of the diaphysis protruded and the bone was stripped of periosteum entirely. I was not certain that the entire epiphysary surface was present, nor did I wait to make a very minute examination of it, as its reduction occupied my attention chiefly. I carried my finger into the wound and on it, as a guide, divided the skin and deltoid muscle vertically until I could reach the upper fragment, and feeling then the surface of the epiphysary cartilage, I was, with some difficulty, able to replace the lower fragment. We now see the

condition of the bone. A small part of the mamellated surface is present
on the diaphysis, and the projecting points of the diaphysis in the centre
and on the inside have been broken off, and are seen lying detached alike
from the cartilage and diaphysis, except that a few shreds of periosteum
still connect them to the cartilage. The periosteum, torn completely
through on the outside, is stripped off the diaphysis down to the insertion
of the deltoid, and even beyond this. It lay in shreds on the inner limit
of the wound, and still retained its connexions to the epiphysis undis-
turbed, the epiphysary cartilage, as it were, preventing any detachment
of it from the upper fragment. In this respect the specimen contrasts
strongly with the femoral injury in which upper and lower fragments
have alike been stripped of their covering to a great extent. We see
here a repetition of the facts noticed in the compound separation of the
lower humeral epiphysis.—*December* 16, 1876.

Hypertrophy of Tibia, causing Lameness.—DR. TYRRELL said: These
two casts of the leg of a boy, aged 16, exhibit in a remarkable manner
the effects of hypertrophy of one leg, caused by disease, producing lame-
ness. I cannot, fortunately, produce the morbid specimen, as the patient
has returned to the country well. He was admitted into the Mater
Misericordiæ Hospital last October, suffering from severe pain in the left
leg—in fact, from periostitis and osteitis of the left tibia. Some two or
three years ago, he said, he had suffered from a similar pain, and that an
abscess formed, and that a portion of bone came away. Afterwards he
remarked that he gradually became lame and that his left leg was longer
than his right, and that in order to prevent himself from limping he had
from time to time to add a piece to the heel and sole of the shoe of the foot
of his right or good leg. Upon examination it was found that the left leg
was two inches longer than the right, and quite straight. The pain was
instantaneously and permanently relieved by perforating the tibia with a
small trephine. The bone was very dense and much thickened. The
trephine was applied over the most painful part. The periosteum was
also very much thickened. The wound healed kindly and quickly, and
he left the hospital in a month, well. The case was one of hypertrophy
depending on increased afflux of healthy blood, but it is a fact worth
noticing that the fibula must have grown *pari passu* with the tibia, other-
wise the leg would have been bent, as occurred in the case figured in
" Paget's Pathology," p. 64.—*December* 16, 1876.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. Moore, M.D., F.K.Q.C.P.

Vital Statistics

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, March 24, 1877.

Towns	Population in 1871	Births Registered	Deaths Registered	Deaths from Zymotic Diseases							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhœa	
Dublin,	314,666	838	826	5	19	20	—	5	22	12	34·1
Belfast,	182,082	535	529	—	27	2	5	24	11	11	38·0
Cork,	91,965	202	189	—	—	—	1	—	1	4	26·8
Limerick,	44,209	79	89	—	—	4	—	1	1	5	26·3
Derry,	30,884	59	52	—	—	—	—	1	1	—	22·0
Waterford,	30,626	88	60	—	—	—	—	—	—	—	25·3
Galway,	19,692	49	51	—	—	—	—	5	—	2	33·5
Sligo,	17,285	21	33	—	—	—	—	—	—	1	24·8

Remarks.

The death-rate was extremely high in Belfast, Dublin, and Galway; it was rather high in the other towns, except Londonderry, in which it was low for the time of year. Omitting the deaths of persons admitted into public institutions from localities outside the registration district, the death-rate of Dublin was 33·2 per 1,000 of the population annually. In London the rate of mortality was 25·5, in Edinburgh 23·3, and in Glasgow 33·1. The zymotic deaths registered in Dublin numbered 112, against 91 in the previous four-week period. Both measles and scarlatina showed a marked increase. The epidemics of measles and whooping-cough continued in Belfast; and the latter disease was also very rife in Galway. Of the 22 fever-deaths registered in Dublin, 6 were caused by typhus, 12 by enteric, and 4 by simple continued fever. Small-pox caused 356 deaths in London, compared with 369 and 381 in the two preceding periods. Thoracic affections became very fatal, owing to the cold weather in March—206 deaths were due to this class of affections in Dublin, compared with 151 and 162 in the two previous periods. In the week ending March 24, bronchitis alone proved fatal in 66 instances.

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20′ *N., Long.* 6° 15′ *W., for the Month of March,* 1877.

Mean Height of Barometer, - - -	29·771 inches.
Maximal Height of Barometer (on 1st at 9 a.m.), -	30·270 ,,
Minimal Height of Barometer (on 25th at 7·30 a.m.),	28·810 ,,
Mean Dry-bulb Temperature, - - -	41·8°
Mean Wet-bulb Temperature, - - -	39·8°
Mean Dew-point Temperature, - - -	37·4°
Mean Elastic Force (Tension) of Aqueous Vapour, -	·225 inch.
Mean Humidity, - - - - -	85·5 per cent.
Highest Temperature in Shade (on 13th), - -	57·2°
Lowest Temperature in Shade (on 1st), - -	27·9°
Lowest Temperature on Grass (Radiation) (on 1st), -	22·8°
Mean Amount of Cloud, - - - -	54·5 per cent.
Rainfall (on 20 days), - - - -	2·741 inches.
General Direction of Wind, - - -	W. and N.W.

Remarks.

This proved to be the coldest month of the winter of 1876–77; and the severe cold, coming at the close of a long winter, told most unfavourably on the " Bills of Mortality " in Dublin. During the first three weeks gradients for W. to N.W. winds held over the British Islands. It often blew freshly from these points of the compass, with showers of cold rain, hail, or snow. On Thursday, the 15th, the coldest period of the winter set in, with fresh, and subsequently light, N.W. winds, and a persistently low barometer. The sky was usually clear by night, so that rather severe frosts occurred. Early on Friday morning, the 23rd, a sudden and rapid fall of the barometer set in, owing to the approach from the Atlantic towards the Irish coast of a serious atmospherical disturbance. The wind backed to S.E. and freshened, bringing a heavy downpour of cold rain. Before 9 p.m. it had again shifted to W. A cold, dry night followed, but, on Saturday morning, a second and still more serious *bourrasque* approached Ireland from the westward. This disturbance was accompanied by an excessive rainfall (1·200 inches), and very inclement weather, the wind rising to a fresh or strong gale from E. The minimal observed reading of the barometer during the gale was 28·810 inches. The weather became mild on the 30th. Rain fell on 20 days, the maximal fall in 24 hours being 1·15 inches on the 24th. Hail fell on 7 days, snow or sleet on 4 days. Soft hail or "graupel" was observed on the 16th. Peals of thunder were heard to N.W. of Dublin at 3·30 p.m. of the 28th.

PERISCOPE.

INTESTINAL OBSTRUCTION SUCCESSFULLY TREATED BY THE INDUCED CURRENT.

M. GIOMMI (*Centralblatt*, No. 30, 1876, and *Racoglitore Med.*, Nov.) gives an instance of the benefits of electricity in intestinal obstruction in the case of a man, aged fifty-one, who, up to the time of his illness, had always been strong and well. He had had complete obstruction for some weeks, and was almost moribund when the idea struck M. Giommi that the cause of the evil lay in atony of the walls of the bowel. He accordingly applied a strong induced current several times during two days, each time for ten or fifteen minutes. One electrode was introduced into the rectum and the other applied to the abdominal wall in the course of the transverse colon. The result was marvellous; copious stools followed the free escape of gas, and after a few days the patient left quite well. [*Conf.* a case, the first we believe on record, in which a similar treatment was pursued by Dr. J. Magee Finny, *Dublin Quarterly Journal*, 1864.]

J. M. F.

DEVELOPMENT OF THE MAMMA.

CREIGHTON's investigations lead him to reject the usually received view of the development of the mammary gland—namely, that it is an invagination of the epithelial elements of the skin by the ingrowing from the surface of tubular recesses of epiblastic cells, which, constantly branching, terminate by expanding into acini. Creighton believes that no such origin can be ascribed to either ducts or acini, but considers that these two structures are developed independently, both of one another and of the epiblast. The ducts are developed much earlier than the acini. Before any acini are observable there may be seen "extending backwards from the nipple certain narrow tracts of cells, which are simply the embryonic cells of the part closely packed together. These tracts of closely packed cells form at various points throughout the embryonic mass independently of each other, and no continuous extension of them can be traced from the nipple." "There is certainly no evidence of a process of growth downwards from the rete mucosum of the nipple. The mode in which a communication is formed between the epidermic excavation or depression of the nipple, and the ducts forming in the lower stratum, does not seem to present any great difficulty of explanation;

but it has been found necessary, for other reasons, to omit it from the present paper." The objects principally used in the study of the development of the acini were fœtal and newly-born guinea-pigs and kittens. In close relationship with the mammæ of these animals is a large and otherwise distinct mass of fat. In the kitten this forms a kind of cushion in the inguinal region, and is readily separable from the gland; while in the guinea-pig the separation cannot be effected without much dissection. The development of the "fat organ" proceeds from the cells of the tissue in which the ducts lie in the manner described by Toldt, and precedes the development of the acini by about three weeks. Some of the cells— namely, those in immediate proximity to the ducts, do not undergo any change then; but, three weeks later, they take on a metamorphosis almost identical with that by which the others were transformed into fat cells, terminating, however, by their conversion into acini, which ultimately open into the ducts. The acini being the essential feature of the gland, so far as secretion is concerned, Creighton regards the mamma as a modified fat-organ.—*Journal of Anatomy and Physiology*, Oct., 1876.

R. J. H.

ON THE TREMBLING IN PARKINSON'S DISEASE (PARALYSIS AGITANS).

M. CHARCOT, in a recent lecture on *paralysis agitans*, particularly insisted on the following points:—1. The name *paralysis agitans* is incorrect. The term *paralysis* cannot be properly applied to an affection in which the muscular power is preserved for a long time. The affix *agitans* is not absolutely correct, because the trembling is absent in some cases in which the correctness of the diagnosis cannot be questioned. He proposes to call the affection *Parkinson's disease*, after the English physician who first drew serious attention to it. 2. M. Charcot maintains that, as a rule, the head and neck do not take part in the tremor which affects the limbs and trunk. In those cases in which the head is observed to tremble, the oscillations are evidently communicated to it from the trunk. To prove this, he fastened a small stick, to the end of which a feather was attached, to the forehead of a patient. When the patient was left alone, the feather was in a state of unceasing agitation; but when the movements of the upper limbs were arrested in some way, as by forcibly elevating the arms and trunk, the feather was perfectly still. This experiment was tried with the same results on several patients. 3. M. Charcot laid particular stress on the fact that tremor is not a necessary symptom of Parkinson's disease. There is a form of the disease in which the tremor is so slight that it is not perceived by the patient, or in which it does not appear till after three or four years, or in which it is even entirely absent. M. Charcot gave in detail the histories of two cases in which all the symptoms of the affection were present and had attained considerable intensity, with the exception of the tremor. This wa-

entirely absent in one of the cases, and in the other was confined to a slight trembling of the left hand, of which the patient himself was entirely unaware. Even this slight tremor was of recent development, while the other symptoms of the disease had existed for four years. In some cases, in consequence of the stiff attitude of the patients, of the extreme slowness of the movements, the expression of hebetude, caused by the immobility of the features, the involuntary flow of saliva, and the interference with speech, the affection has been mistaken for softening of the brain. Usually when this error has been made, the rigidity was especially marked on one side. The intellectual faculties, however, remain intact in Parkinson's disease.—*Le Progrès Médical,* December 2, 1876, and *N. Y. Med. Record.*

A NEW METHOD OF REGISTERING THE VARYING SIZE OF THE BLOOD-VESSELS IN MAN.

DR. A. MOSSO has invented an instrument for this purpose. He calls it a *Plethysmograph.* It consists of a cylindrical glass vessel open at one end, and communicating at the other with an India-rubber tube. Through the open end the part to be observed (the arm) is introduced and the opening is then closed hermetically by an India-rubber fastening, which slightly compresses the limb as it passes into the cylinder. The India-rubber tube passing from the other end of the cylinder terminates in a piece of glass tubing, bent at right angles, and which dips into a test-tube containing water. The cylinder, and India-rubber, and glass tubing are completely filled with water, except the space occupied by the limb under examination. The test-tube is supported by cords passing over pulleys and counterpoised by a weight, to which is attached a writing point, which traces, on a rotating drum, the movements the test-tube under-goes. The test-tube hangs in a beaker of water. It is clear that if the arm increases in bulk, from distension of its vessels, part of the water in the cylinder will be displaced and will pass into the test-tube. This will consequently increase in weight, and will sink until it has displaced as much water in the beaker as has passed into it from the cylinder; while if the quantity of blood in the arm diminishes, fluid will be drawn into the cylinder from the test-tube, which will consequently ascend. It was found that any mental excitement, even that caused by the entry of Prof. Ludwig into the room, caused a marked diminution in the bulk of the arm. A similar effect was produced by efforts of mental arithmetic, such as multiplication of three figures by two. When the subject of experiment was at rest, and submitted to no excitation, the volume of the arm remained unaltered; but if he made a deep inspiration, or if he held his breath, the volume of the arm immediately diminished. This diminution is not due to aspiration of blood from the vessels by the respiratory movement, but to the action of the vaso-motor nerves, whose

centre is in intimate relation with the respiratory centre; and also perhaps to an accumulation of blood in the lungs during a deep inspiration, which blood is only slowly restored to the general circulation. Electric stimulation of the arm also caused diminution of its bulk, different according as the stimulus was feeble or severe. Compression of the brachial artery caused diminution of bulk, followed by marked increase when the compression was removed. Other experiments were made on the effects of stimulation, electric and thermic, applied not to the arm itself but to other parts of the body.—Analysis in *Archives de Physiologie*, 1876, p. 175.

J. M. P.

PERITONITIS IN CHILDREN.

In this disease, which is often extremely difficult of diagnosis, S. Kersch (*Memorabilien*, 6 *Heft*, 1876) calls attention to two symptoms which are quite characteristic of the disease. In the first place there is the impossibility of lying with the legs stretched out, the patient having both thighs flexed upon the abdomen, and, consequently, he cannot be made to stand on his feet. The second important aid to diagnosis is the respiration. Even before a peritoneal exudation is demonstrable, sharp pains, which arise in both hypochondria, make inspiration very difficult; and, should there co-exist a bronchial catarrh, coughing is impossible. Expiration goes on unimpeded. Such children can cry quite loud; but the pauses between the cries are long, being filled out by a series of very short inspiratory acts. This difference between inspiration and expiration during crying is the most important diagnostic symptom in the peritonitis of children, and is never absent. The prognosis is less unfavourable than with adults. Although a subsequent cheesy infiltration of the lungs is to be feared, yet this process with young children has not the same serious significance as with elder persons. In such cases, when the child receives proper care and treatment, cicatrisation or calcification of the infiltration often takes place. In forming the prognosis the author's many years' experience has demonstrated that the sex of the patient must be taken into consideration, as with little girls sterility remains behind notwithstanding they may otherwise be restored to perfect health. The author has observed ten such cases between five and fourteen years of age.—*Boston Med. & Surg. Jour.*, Feb.

AN UNIVERSAL INDEX OF MEDICAL LITERATURE.

DR. JOHN G. BILLINGS, of the United States Army, and now in charge of the Library connected with the Surgeon-General's Department in Washington, is engaged in a work of the utmost importance to medical science. He is bringing out, at the expense of Congress, an Index of the Medical Publications of the Old and New Worlds, by reference to which those who are engaged in any particular study may find the titles and dates, not only of all treatises on the subject which have been published, but of the various papers which have appeared in the medical journals. A specimen of this Index reached us some time ago, and for completeness it bids fair to excel anything of the kind which has ever been attempted. Great difficulty has been found in supplying some gaps in the series of journals of by-gone years. Booksellers cannot procure them, and they are only to be had through private sources. We publish a list of some which we have little doubt lie neglected on book-shelves in the libraries of some of our medical friends; and we hope, if they can find them, they will present them to the Washington Library, so as to complete the series. The Surgeon-General of the United States Army sends copies of the publications of his office free of charge to the medical journals of this country, and they are to be found in our medical libraries in this city; and it should be our pleasure, as it certainly is our interest, to make this great work of reference as complete as possible. If any of the missing volumes are sent to the publishers of this Journal, all charges will be defrayed, and the volumes forwarded to Washington.

Dublin Journal of Medical Science. Bi-monthly. 8vo. 2 Vols. annually. Want Nos. 48, 49, 52, 53, 55, 56—that is, No. 3., Vol. XVI., January, 1840. No. 1., Vol. XVII., March, 1840. Nos. 1, 2, Vol. XVIII., September and November, 1840. Nos. 1, 2, Vol. XIX., March and May, 1841.

Liverpool Medical Gazette, &c. By H. LANE. Monthly. Commenced about 1833. Want all or any part.

Liverpool Medical Journal. Monthly. Commenced about 1834. Want all or any part.

Medical Quarterly Review. By J. NORTH and G. F. BURNETT. Vol. IV., 8vo. London, 1833–4. Want all or any part.

Medical Times. Weekly. London. Want Vols. I., II., September 28, 1839, &c. Whole Nos. 94, 95, 97. Vol. IV., July 10, 17, 31, 1841.

Medical and Chirurgical Journal and Review. By JAMES JOHNSON. London. Commenced in 1816; continued in 1819 as *Medico-Chirurgical,* or *London Medical and Surgical Review.* Want Vol. II., July–December, 1816; also Vol. II., Quarterly Series, July, 1819, April, 1820.

Monthly Archives of the Medical Sciences. By DR. HUNTER LANE. 8vo. London and Liverpool. Commenced about 1833. Want all or any part.

Provincial Medical and Surgical Journal. By N. GREEN, and others. Weekly. London. Want Vols. II., I., 1840–41.

PART I.

ORIGINAL COMMUNICATIONS.

ART. XVIII.—*Lessons from Surgical Practice.* By B. WILLS
RICHARDSON, F.R.C.S.I.; Senior Surgical Examiner in the
Royal College of Surgeons; and Surgeon to the Adelaide
Hospital, Dublin.

I. SPONTANEOUS ANEURISM OF THE RIGHT POSTERIOR TIBIAL
ARTERY CURED BY COMPRESSION.

II. SPONTANEOUS ANEURISM OF THE RIGHT SUPERFICIAL
FEMORAL ARTERY IN HUNTER'S CANAL, CURED BY COM-
PRESSION; ENLARGED SPLEEN AND LEFT LOBE OF LIVER;
CLOSE STRICTURE OF THE URETHRA.

THE following cases of idiopathic aneurism will, I am sure, interest
the practical surgeon, the cures having been effected by the com-
pression method—a method of which every Irish surgeon should
feel proud, so much lustre having been shed by it on the Irish
School of Surgery.

CASE I.—*Spontaneous Aneurism of the Right Posterior Tibial Artery, with
permanent patency of the Aortic Valves.*—W. C., aged nineteen years, having
a remarkably anæmic appearance, was admitted to the Adelaide Hospital
on the 24th November, 1873. For the two preceding years he had been
subject to attacks of pain in the sole of the right foot, and to occasional
swelling of the right leg from just below the knee to the ankle. Twelve
months before admission he was kicked on the tuberosity of the right
tibia, which, however, caused no inconvenience; and ten weeks before

admission he was suddenly, to him unaccountably, seized while seated at his occupation, that of watchmaker, with a darting pain in the calf of the right leg, which increased in severity as the day wore on. The day following this seizure he went out for a short walk, with the hope of obtaining relief. The pain, on the contrary, increased, and in a few days swelling of the leg occurred.

When the pain had continued a month it increased still further in severity, caused fainting, and compelled him to take to bed, where he remained for ten days, and had medical treatment. This apparently controlled the pain, the swelling partially subsided, and he was enabled to take short out-of-door walks. The pain he experienced in the sole of the foot seemed about this period to have changed into a burning sensation. Shortly afterwards the calf again swelled, and severe pain recurred in it. For the six days before admission he was confined to bed, and finally was admitted under Dr. Walsh.

Symptoms on admission.—The upper part of the calf of the right leg was enlarged by a pulsating tumour that barely encroached upon the lower part of the popliteal space. Wherever it was within reach of the fingers its systolic distension was uniform, excepting just below and behind the insertion of the sartorius, gracilis, and semitendinosus muscles, where it seemed to have most force. A loud systolic murmur was audible in the tumour. When the blood-current in the superficial femoral artery was arrested by pressure, pulsation ceased and the murmur was no longer audible. The tumour was evidently an aneurism partly consolidated, that could only be reduced to half its calibre by thus shutting off the supply of blood. The leg measurement, taken at the most prominent part of the aneurism, showed a circumference of 13⅜ inches, whereas the circumference of the corresponding part of the opposite leg was but 9¼ inches. A branched slightly enlarged vein ramified over the tumour. The patient suffered severely from the old pains, particularly at night-time.

The visible pulsation of the arteries of the neck and of other superficial arteries led to the suspicion of heart complication ; and on examination it was found that the area of the cardiac dulness was much increased, and that the double murmur of aortic permanent patency was well marked, being very audible over the greater portion of the post-sternal region. The pulse averaged 100 in the minute.

25th November.—He was ordered full diet and a mixture composed of iodide of potassium, infusion and tincture of cinchona bark. An ice-bag was placed on the aneurism.

28th November.—Several of our hospital pupils commenced digital compression of the femoral artery in Scarpa's space at 11·45 a.m., each gentleman doing duty for half an hour. The pressure was well borne for the first three hours, after which severe intermittent pains shot from the knee to the ankle.

29th November.—Compression was steadily maintained during the night, but he was very restless, and suffered much pain. Had seventy-five grains of hydrate of chloral sub-divided into four doses, with intervals between the doses. He awoke, however, whenever the digital compressor was changed. In the afternoon the aneurism felt more consolidated, and the pain had greatly diminished. He was given in the evening a compound rhubarb pill and a hydrate of chloral and liquor muriatis morphiæ draught.

30th November.—The digital pressure had to be made alternately at the groin and a little above the entrance to Hunter's canal, the skin of the groin being intolerant of continued pressure. He then experienced but little pain, and at 4·50 p.m. the compressing pupil reported that the aneurism had ceased to pulsate; 8·30 p.m. Dr. Walsh examined the aneurism and could not detect any pulsation. He accordingly requested that the pressure should be applied for only half of every hour. 11 p.m., recurrence of distinct pulsation in the aneurism in thirty minutes after the discontinuance of the last digital compression.

1st December.—Aneurismal pulsation was distinct, but could be effectually controlled by the pressure; pain trifling.

2nd December—Complete digital compression was maintained during the night without disturbance of sleep, but at 10·30 a.m. he was seized with a severe pain in the crest of the right tibia. Consolidation of the aneurism increased; slight œdema of the leg. A flannel roller was applied, with relief, from toes to knee; the iodide of potassium and cinchona mixture was repeated. Complete digital compression continued.

3rd December.—He became restless in the night, and removed the flannel roller, insisting that it either caused or increased the numbness in the leg. When the pressure was omitted for a few moments the pulsation, although slight, continued to be most distinct near the insertions of the sartorius, gracilis, and semitendinosus muscles. Ordered a mixture composed of syrup of iodide of iron, iodide of potassium and water. Cabbage was added to his dinner ration, his gums being spongy. Complete digital compression was continued and alternated in position as already mentioned.

4th December.—He was restless till 2 a.m., and was given 15 grains of chloral hydrate, which caused an hour's sleep, then he awoke and complained greatly of stinging pain along the external side of the right tibia. He was at once given 20 grains of the chloral hydrate, and had three and a half hours' sleep. He became impatient of the digital compression, and expressed a wish for its discontinuance. Dr. Walsh, too, was dissatisfied with the results obtained from it, for, although consolidation of the aneurism had increased, he considered that persistent instrumental compression would probably lead to more decisive results. He therefore directed the digital compression to be discontinued at 10

p.m., and gave the patient the chance of a good sleep previous to the application of a ring tourniquet on the following morning.

5th December, 11 a.m.—The blood-current through the superficial femoral artery was completely stopped with Skey's ring tourniquet applied a little above Hunter's canal, but great venous congestion of, and pain in, the limb below the seat of compression followed. A Carte's compressor, placed in the same position, was substituted for it. This instrument likewise, seeming to cause much pain, was removed at intervals with great relief to the patient. He was able, on one occasion only, to bear its action for two consecutive hours. Circumference of leg, taken at the most prominent part of the aneurism, 15 inches.

6th December.—He had fifteen grains of hydrate of chloral in a draught at 11 p.m., and at once fell to sleep, but soon awoke in extreme pain, and at 1·30 a.m. the instrument had to be removed. Sleep followed until 7 p.m.; ten drop doses of tincture of digitalis were added to each ounce of his mixture. The leg was loosely bandaged up to the knee. The aneurism was very firm, but still had an even continuous pulsation. Its size had not altered.

7th December.—During the night the superficial femoral artery was compressed with Carte's and Read's tourniquets alternately. The patient slept tolerably well. At day-time the pain was not so severe.

8th December.—The patient had become so restless at night it was difficult to maintain the instruments in position. Ordered saccharated carbonate of iron and extract of hyoscyamus in pill three times daily.

9th December.—Instrumental compression discontinued for four hours during the night. Circumference of leg, taken at the most prominent part of the aneurism, 15⅝ inches, yet its feel was firm, and encouraged the belief that stratification of fibrine in the sac had sufficiently increased to justify the continuance of the compression. Dr. Carte, who saw the patient on this day, held a similar view.

10th December.—No change in the aneurism; patient had a good night.

11th December.—Another good night's rest. Circumference of leg, taken at the most prominent part of the aneurism, 15¼ inches. Dr. Carte himself applied his compressor.

13th December.—The pressure was maintained; and during the evening of the 12th Carte's compressor was applied to the femoral artery mostly in Scarpa's space. At 11 p.m. the patient was awoke by excruciating pain in and around the aneurism, which had much increased in solidity, but yet pulsated distinctly. The compressor was removed for a little time, was then reapplied in upper part of Scarpa's angle and at top of Hunter's canal alternately. Drs. Walsh and Carte agreed that henceforth compression should be tried during the day only, and limited to Scarpa's angle. Circumference of leg, taken at the most prominent part of the aneurism, had reduced one quarter of an inch.

Fig. 1.
Spontaneous aneurism of the right posterior tibial artery.
Case of W. C., aged nineteen years.

14th December.—Had a quiet night. The aneurism continued firm, and the compression was maintained almost uninterruptedly from 9 a.m. to 8 p.m.

20th December.—Circumference of leg, taken at the most prominent part of the aneurism, 14 inches, being a reduction of 1⅜ inches. The aneurism remained remarkably firm, but its pulsation was equable or continuous. Patient was allowed out for an hour's drive.

25th December.—The pad of the tourniquet having become disarranged was sent for repair, and was not reapplied until the 24th December. In the meantime compression with weights was carried on in Scarpa's angle. The aneurism, as regards calibre, had not, however, improved; for measurement of the circumference of the leg, taken at the most prominent part of the aneurism, showed an increase of half an inch.

Throughout the following months of January, 1874, and February, no appreciable change in the aneurism. The patient was allowed out for an hour's drive on fine days, and with great benefit to his general health.

8th January.—Being under my care during the absence of Dr. Walsh from Dublin, I had a cast taken of the aneurismal leg (Fig. 1.),* the patient being then in a favourable state for the process.

26th January.—Circumferance of leg, taken at the usual part, 13¾ inches; and on the 15th of February, 13⅜ inches. The aneurismal pulsation continued strong and equable until the 18th, when it suddenly weakened while the patient was out for a drive. Pulsation ceased altogether on the next day, but it was thought advisable to continue the compressing treatment for a few more days. The aneurism had become of the so-called stony hardness, and more globular in shape.

The patient left hospital on the 13th April, the cured aneurism having considerably reduced in size, and his general health and spirits being much improved. The sounds of the aortic valve insufficiency had not altered in intensity. Mr. (now Dr.) J. Dunbar Dickson, our then resident pupil, whose assiduity in watching the case is above all praise, and who carefully noted the periods of both the digital and instrumental compression, informed me that the digital compression was maintained continuously for 154 hours, and that the two forms of compression extended over a period of 115 days before the final cessation of pulsation.

2nd December, 1874, W. C. was again admitted to hospital, in much alarm from some symptoms referrible to the left shoulder and arm. A fortnight previously he was suddenly seized with pain in the supraspinous region of the left scapula, which gradually extended to the forearm, and finally located itself about two inches below the internal condyle of the humerus. Neither swelling or tumour was discernible at this spot. The pain, however, was so severe as to prevent sleep, and required morphia in large doses to overcome it. Knowing the great aneurismal

* Fig. 1.—The cast is in the Museum of the Royal College of Surgeons, Dublin.

predisposition that has been occasionally observed in that condition of the arterial system in patients afflicted with aortic valve insufficiency, it was thought that possibly the ulnar artery had become cylindrically dilated at the strongly pulsating point. Curious to say, pressure of the brachial artery a little above the elbow led to immediate and permanent cessation of the pain. There was no appreciable change in the cardiac symptoms. He, however, returned home to the country, where he succumbed in a few months, and the opportunity for examining the heart and arterial system was therefore lost.

May I not, without fear of being accused of exaggeration, say, that this case was a great triumph for the "compression treatment of aneurism," the patient being so bad a subject for almost any kind of cutting operation? Need I further add that arterial ligation would have had but little chance of causing occlusion of the tied portion of vessel? I fancy there are few operators of sound pathological knowledge who, in a case like this, would ligate without great apprehension of the ultimate result, owing to the extreme probability of unhealthy action supervening at the tied part. A pallid patient, with irritable, bounding, visible pulsating arteries, and with a heart largely dilated, is one on whom the immediate compression of arteries by either the ligature or other occluding means should be practised under the demand of necessity only.

An interesting element in the case was the cessation of pulsation for a few hours on the 30th November, which was probably caused by temporary occlusion of the orifice of entrance or of exit of the sac by a detached piece of fibrine. If this were so, and the occluding fibrine was situated at the orifice of exit, it evidently did not leave the sac, as there was no subsequent evidence of embolic occlusion of any of the distal arteries.

With regard to the force of the compression followed, it should be observed that when practicable the blood-current in the superficial femoral artery was altogether stopped for a considerable portion of the treatment, particularly when it was digital. But when it was instrumental the complete stoppage of the current was oftentimes impossible, and therefore the two great principles as to force of compression were practised—*i.e.*, the complete and the partial stoppage of the blood-current in the artery leading to the sac. I agree with those who consider it a great mistake in cases in which the treatment is likely to be prolonged, to confine the patient to the house, gentle out-of-door carriage exercise, when not contra-indi-

cated, being frequently of assistance in the accomplishment of the cure.

As it is possible when pulsation of an aneurism ceases, while a patient is walking, that a particle of fibrine has been displaced and forced by the blood-current to the outlet of the sac, and thus occlude it, there is the same risk which attends upon the method of treatment of aneurism by manipulation—namely, the forcing, by an awkward movement of the limb, of this piece of fibrine into and along the artery leading from the sac, embolism and its consequences being the result. It is for this reason that carriage is preferable to walking exercise.

The case has another interesting element in the fact of its having been an idiopathic aneurism of the posterior tibial artery, which is but seldom seen, few cases of spontaneous aneurism of this artery having been recorded.

The following case also, was a most unfavourable one for arterial ligation, and may likewise be considered a very brilliant example of successful compression treatment:—

CASE II.—*Aneurism of the Superficial Femoral Artery in Hunter's Canal— Enormous Splenic Enlargement—Enlargement of Left Lobe of Liver—Close Organic Stricture of the Urethra—Blindness of both Eyes.*—A. M'C., aged forty years, was admitted to the Adelaide Hospital on the 19th July, 1875. He stated that his legs became painful and swollen nine to ten years previously, and were cured by medical treatment. A few years later he discovered a swelling in the left side of his abdomen, and suffered occasionally from palpitation. He thought that the abdominal swelling had not undergone any change since its first discovery.

Eight weeks before admission he was attacked with severe pain in the right knee, and shooting pains in the corresponding thigh, which he attributed to rheumatism; and on one occasion while his wife was rubbing the thigh, with the view of relieving him, she detected the aneurism. He was at once brought to hospital. The abdomen was carefully examined, and it was found that the tumour in its left side was apparently formed of the spleen. It was smooth on the surface, and extended from immediately beneath the left side of the diaphragm to two inches from the crest of the ilium and to about two and a half inches from Poupart's ligament, as shown by the dotted line *a*, Fig. 2. The edge represented by this line was thin, allowed the finger to be partially insinuated under it, and when the tumour was fully distended, its most convex portion reached a position corresponding to the left side of the umbilicus. The tumour varied in size, being largest after meals, and

was always painless. He never had epistaxis, nor any group of symptoms that could be called a complete ague fit. The liver descended below the ribs, as represented by the dotted line *b*, Fig. 2. The heart's sounds were normal. The aneurism, which was then about the size of a small orange, had not passed the limits of Hunter's canal. Its pulsations were synchronous with those of the heart, and were uniform or equable in every part within reach of the finger. A loud systolic murmur was audible in it; and compression of the femoral artery in the groin almost completely emptied the sac. A little above Poupart's ligament, of the aneurismal limb, two to three small indurated glands could be felt just outside the brim of the true pelvis (dotted line *c*, Fig. 2.)

Fig. 2.
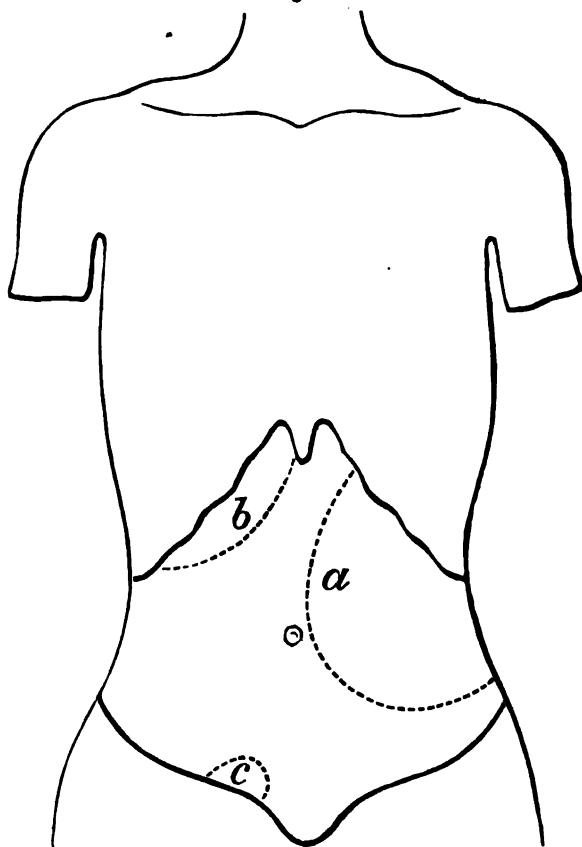
a. Dotted line to represent the right and the lower edge of the enlarged spleen. *b.* Edge of liver which was inclined towards the splenic tumour. *c.* Enlarged lymphatic glands.

On further questioning the patient it was found that he had organic stricture of the urethra, which my urethrometer[a] proved to be $3\frac{1}{2}$ inches from the orifice of the meatus. The stricture would allow only a No. $\frac{1}{2}$ bougie to pass it. The urine, moreover, deposited muco-pus. His blindness was traumatic.

21st July.—A Read's modified Signoroni's tourniquet was applied at 12·48 p.m., to the superficial femoral artery, just below its origin, and a flannel roller was put on the limb. The compression being very painful could not be made complete, and was tolerated for two hours only. The aneurism, which pulsated feebly during the compression, again pulsated strongly.

22nd July.—A Carte's compressor was applied at 11·50 a.m. to the femoral artery in the same situation. The compression was complete, and the aneurismal pulsation at once ceased. Pain being very severe the instrument had to be removed in two hours. Difficulty and frequency in micturition having become distressing, a No. $\frac{1}{2}$ soft bougie was introduced, and allowed to remain à demeure for a few hours.

23rd July.—Slight attack of urinary fever, and the Carte's compressor was not used. Weights were, however, applied to the common femoral artery in the following manner:—A leaden 6-lb. weight (Fig. 3), kindly lent to me by Mr. Tufnell, and shaped like the anterior portion of the

Fig 3.
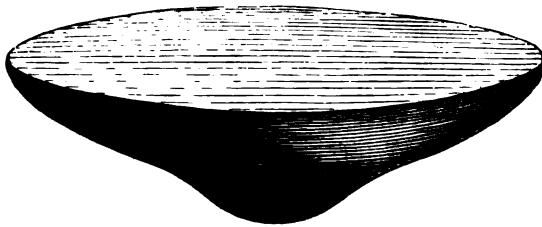
Leaden weight for compressing the femoral artery. Real size—long diameter at upper flat surface, $5\frac{3}{4}$ inches ; short do., $4\frac{1}{4}$ inches ; distance from centre of upper flat surface to most prominent part of nipple, $1\frac{3}{4}$ inches. This represents the greatest thickness of the weight.

mamma, having a projecting nipple, was placed in the groin, the nipple directly pressing the common femoral artery. On this weight an ordinary 2-lb. iron weight was laid; one of our most attentive pupils Mr. (now Dr.) William M. Wright having undertaken to superintend this form of compression while he remained in town. It was commenced at 12·30 p.m. Mr. Wright assisted the weights with slight hand pressure, and thereby completely stopped the aneurismal pulsation.

[a] The illustrated description of this instrument will be found in the number of this Journal of November, 1873.

The patient would not submit to the compression beyond 3 p.m., and the weights were removed. Milk diet.

24th July.—Carte's ring-compressor was reapplied at noon to the superficial femoral artery in Scarpa's angle. The compression was sufficiently complete to stop all pulsation in the aneurism, but the pain being severe the instrument had to be removed in two hours.

25th July.—Same compressor reapplied at noon in same situation, and the aneurismal pulsation was completely controlled, but pain being again severe the instrument was removed in two hours.

26th July.—Carte's compressor changed for the weights (8 lbs.), the nipple having been placed over the common femoral artery, just below Poupart's ligament. Complete compression was thereby maintained for two hours and fifty minutes, when increasing pain demanded their removal.

27th July.—The weights were increased to 10 lbs. by substituting a 4-lb. iron weight for the 2-lb. one, and at 12·30 p.m. were applied to the common femoral artery, the nipple being directly over the vessel. They were removed in forty-five minutes to allow the patient to have his dinner; and were reapplied in the same position at 2 p.m. The aneurismal pulsation was completely controlled for two hours, when the patient removed them.

28th July, 12·50 p.m.—Weights reapplied in same situation, but were removed in forty minutes to enable the patient to dine. They were replaced at 2·15 p.m. for one and three quarter hours. At 5 p.m. they were replaced in position for two hours. This day's compression, although interrupted, was complete.

29th July.—Weights applied at 11·10 a.m. and compression for two hours, but not uninterruptedly complete. They were replaced at 2·30 p.m., and compression for two hours and a half, but not always complete. Whenever the weights were kept in position longer than usual, the patient allowed them to glide partially off the femoral artery, which resulted in recurrence of feeble pulsation in the sac. He was unfortunately rather unmanageable, and it being holyday time our students were too few for even this interrupted compression.

1st August.—The same weights were applied at about 10½ a.m., and for the greater part of the time complete compression was maintained until 9 p.m.

2nd August.—Same weights applied at 10 a.m., and complete compression, with scarcely an exception, was maintained for the greater part of the day. The aneurism had become firmer, with more controllable pulsation. Moreover, it could not be emptied so completely of blood as before compression was commenced. Meat diet.

5th August.—The weights were used for six and three quarter hours and caused but little pain. The compression was not always complete.

Pulsation continued strong and equable, yet the increasing firmness of the aneurism was encouraging.

6th August.—Complete and incomplete compression this day for four and a half hours. He now endeavoured to manage the weights himself, but with difficulty, owing to his blindness.

7th August.—Compression of the same characters at intervals for seven hours and a half.

8th and 9th August.—Fourteen and three-quarter hours interrupted compression of the same characters.

10th August.—It was doubtful whether the pulsation had not become stronger. At all events the sac was firmer, and evidently contained more fibrine. The weights were increased to 11 lbs., and interrupted compression was maintained for five and a half hours. It was unavoidably incomplete. The flannel bandage was carefully adjusted.

11th August.—Same description of compression for five and a quarter hours, but was not continuous.

15th August.—Compression since the 11th inst. for about the same number of hours daily, but on this day it was continued for eight hours. The patient, with a little management, could now control the pulsation with weights of 7 lbs.

12th, 13th, and 14th August.—About same amount of interrupted compression.

15th and 16th August.—Compression of about thirteen hours.

17th August.—Nine hours' compression of same character, but the pulsation of aneurism was very strong, and required weights of 10 lbs. to control it.

18th August—Seven and a half hours' compression of same character.

19th August.—Patient managed the compression badly, and it was very incomplete.

20th, 21st, 22nd, and 23rd August.—Twenty-seven hours' compression.

24th and 25th August.—Sixteen and a quarter hours' compression.

26th August.—Five hours' incomplete compression by the patient. The aneurismal pulsation continued equable and strong, but the sac remained remarkably firm. Same weights. I passed a No. 1 conical bougie into bladder and at once withdrew it.

1st September.—Compression for nine hours daily since the 26th ult.

16th September.—Compression for several hours daily since the 1st inst., but the patient managed it carelessly, and it was imperfect. The aneurism was apparently more solid. He was given a grain each of acetate of lead and sulphate of iron, with half a grain of powdered digitalis in pill three times daily.

22nd September.—Compression for the same number of hours daily since the 16th instant.

8th October.—Since the 22nd ult. the daily amount of compression was nearly the same. The patient having become impatient of the weights, one of Carte's compressors was applied to the superficial femoral artery in Scarpa's space at 12·30 p.m., but he submitted to it for only an hour, being the whole compression of this day. It was, however, complete.

9th October. Complete compression with the weights.

10th October.—Complete compression with Carte's instrument for one and three-quarter hours.

11th October.—Two and three quarter hours' incomplete compression with Carte's instrument. Having some colicky pains the acetate of lead was omitted from the pills, and he was given a purgative.

12th October.—Carte's compressor for 2·25 hours.

13th October.—Carte's compressor applied for a few hours. Aneurismal pulsation continued strong and equable when not under the influence of pressure. Firmness of sac persistent.

14th to 18th October.—Thirty-two hours complete compression with Carte's apparatus.

19th to 22nd October.—Forty-eight hours' compression with same apparatus.

Ordered a mixture composed of iodide of potassium, bromide of potassium, paregoric elixir, glycerine and water.

23rd to 28th October.—Sixty hours' compression with the Carte's compressor. The dose of iodide of potassium in the mixture was increased to ten grains three times daily.

29th to 31st October.—Thirty-six hours' compression.

1st and 2nd November.—Twenty-fours' compression.

3rd November.—Had a severe rigor after the use of the bougie. The doses of the iodide and the bromide of potassium were increased to twelve grains each, three times daily. Twelve hours compression with same apparatus.

4th to 7th November.—Forty-eight hours' compression.

8th November.—Seized with a severe rigor at 8.30 a.m., which lasted ten minutes. At 10·45 a.m., the pulse was 100, and temperature in axilla 102·5°. Tongue unusually red. A urethral instrument had not been used for five days. Compression was discontinued, and the hospital diaphoretic with tinctures of veratrum viride and aconite was substituted for the iodide of potassium mixture. Milk diet. Marsala.

9th November.—Did not sweat after the rise in temperature. Pulse 72. The bowels had been freed, and tongue was paler. The aneurism had become more convex, and yet withal was remarkably firm. Its greatest transverse diameter, taken with the calipers, within reach of accurate measurement, was 4½ inches. The pulsation continued strong and equable, and yet a 6-lb weight with a little management effectually

Fig. 4.
Spontaneous aneurism of superficial femoral artery in Hunter's canal.
A. M'C., aged forty years.

controlled it. The patient feeling himself much better, resumed the regular compression with the 6 and the 4-lb. weights. They were, however, applied to the external iliac artery, a little above Poupart's ligament. The aneurismal prominence being well marked this day I took a drawing of it. (Fig. 4.)

10th to 15th November.—Thirty hours' compression on the same part of external iliac artery. He resumed the iodide of potassium mixture on the 10th November.

16th November.—A No. 3 solid silver bougie was passed into bladder. From this date to the 14th December two hundred and three hours' compression of the external iliac artery with the 10-lb weights, and on the latter day the application of an ice-bag over the aneurism was commenced.

14th to 28th December.—One hundred and five hours' compression of the external iliac artery. Each day the ice-bag was applied over the aneurism for four hours.

29th and 30th December.—Fourteen hours' compression of the external iliac artery. I fancied this day that the aneurism had extended its area corresponding to its deep relations. Its pulsation was still equable and strong, and although it was remarkably firm I became uneasy, and proposed the " ligature" to him in case a more decided improvement did not follow upon an increase in the weight pressure to be added the following day.

31st December.—A 7-lb. was substituted for the 4-lb. weight, and placed upon the 6-lb. leaden weight, which itself was applied to the external iliac artery for eight hours, and thereby the pulsation was effectually checked.

1st and 2nd January, 1876.—Sixteen hours' continued compression.

3rd January.—One of our most intelligent pupils, Mr. (now Surgeon) James M'Munn, undertook the management of the compression, and maintained it uninterruptedly with the same weights day and night. After some hours of this continuous pressure, Mr. M'Munn, wishing to ascertain its effect, removed the weights for a moment, and had the satisfaction to find that the aneurism had become perfectly solid and completely devoid of pulsation. He readjusted the weights in the same position.

4th January.—A momentary return of pulsation in the aneurism early in the morning, which ceased when the weights were reapplied to the external iliac artery.

I saw the patient at 1 p.m. The aneurism was perfectly free from pulsation, had become globular in shape, and extremely hard. There was no pain in the limb. The weights were kept in position for a few hours longer.

5th January.—The patient, of his own accord, reapplied the weights

to the external iliac artery at 8 a.m., and kept up pressure for two and a half hours. A careful examination of the limb was now made, with the following results:—The femoral artery pulsated strongly down to an inch from the aneurism, between which point and it no pulsation could be detected. The diameter of the aneurism, taken with the calipers, was four and a half inches, the diameter it had on the 6th November. No pain in leg. The ice-bag was reapplied for two hours to the tumour on the evening of the 4th, and was repeated on this evening. Between the 5th and 12th January the aneurism if it possibly could have been harder, had become so. He had a severe seizure of lumbar pains, which were relieved by morphia hypodermically. He was, however, at the same time taking a mixture composed of iodide and bromide of potassium, tincture of veratrum viride, and infusion of cinchona bark.

13th January.—The treatment of the urethral stricture was recommenced, and a No. 3 conical bougie was easily passed. Some benzoate of ammonia was added to the mixture.

16th January.—The aneurism had reduced an inch in diameter. On the 17th January some of his blood was examined microscopically for excess of white cells, but it could scarcely be said that they were present in abnormal number. There was no œdema of the leg, but its temperature was four degrees lower than it was in the opposite one. Pulsation had not returned in the dorsal artery of the foot. Indeed it had never been felt since his admission to hospital.

From the 16th to the 22nd January the treatment of the stricture was favourable. The temperature of the right foot was now only one degree below that of the opposite leg.

From the 22nd to the 29th January the treatment of the stricture was continued.

31st January.—Aneurism reduced to three inches in diameter.

2nd February.—No. 7 pewter bougie introduced without difficulty. A rigor, followed by a hot stage only, occurred in the evening.

8th February.—Treatment of the stricture resumed, and a No. 8 conical bougie introduced. Between this date and the 6th March the treatment of the stricture was most satisfactory. But having on the evening of the 5th accidentally set fire to his bed-clothes, I was compelled to send him home. He was an inveterate smoker, and could not be induced to omit smoking in the ward during the night, to the great annoyance of many of the other patients.

It will, I apprehend, be conceded by every reflecting surgeon whose reason is not clouded by the *cacoethes scalpelli*, unfortunately now too rampant, that this was about as fortunate a case as could be met with in practice. An enlarged spleen, an enlarged left lobe of the liver, a tight stricture of the urethra, with · muco-purulent

urine, and possibly diseased kidneys, are not likely to increase the chances of the supervention of healthy action at the seat of ligation of an artery. Indeed, I have known the presence of renal disease alone to have for its consequence the failure of any attempt at the adhesive closure of an artery at the seat of the ligature.

I determined then, no matter how long the compression treatment might occupy, to refrain from the ligature, provided increasing firmness in the aneurism, be it ever so slow, encouraged its continuance. Fortunately it was crowned with success, and an operation has been obviated, which, if we may argue from probabilities, would have been rapidly fatal.

ART. XIX.—*Studies in Medical Spectroscopy.* By CHARLES ALEXANDER MACMUNN, B.A., M.D., Univ. Dub.

I. A NEW METHOD OF MAPPING ABSORPTION SPECTRA.

II. A NEW METHOD OF RECORDING THE BREADTH AND POSITION OF ABSORPTION BANDS.

III. ABSORPTION SPECTRA OF BLOOD, BILE, AND URINE.

THE accounts of the absorption spectra of blood, bile, and urine, as they are given in the text-books of Physiology, are very disappointing to anyone anxious to know something about them. The methods of proceeding, in order to procure the blood spectra, differ in different books, and in very few is there any mention whatever of the spectra of bile and urine. In the present paper I hope to be able to show with what readiness almost all the blood spectra hitherto described can be procured; to describe two new spectra, and briefly to refer to the spectra of bile and urine; also, to bring before the notice of my medical brethren two new methods of mapping and recording spectra, which I believe may prove useful to them. Before describing them, a few words on the instrument itself and its applications may not be considered out of place.

An account of Newton's experiment, performed 200 years ago, and of the improvements made upon it by Wollaston and Simms until a pure spectrum was obtained, will be found described in the text-books of Physics.* It will suffice to say here that a spectroscope consists essentially of a slit to admit a pencil of light, a prism in which it is refracted and dispersed, and a lens placed between them which renders the rays parallel before entering the prism.

* See Deschanel's Natural Philosophy, and Ganot's Physics, &c.

In the microspectroscope,[a] which is useful when the quantity of material to be examined is small, the spectrum is viewed directly by the eye. The microspectroscope is a direct vision instrument. To accomplish this, three or five prisms are employed, made of flint and crown glass, by the suitable combination of which deviation is eliminated, while sufficient dispersion is retained; consequently the spectrum is short, but the definition is good. Its use is attended with inconveniences which are not experienced with the chemical spectroscope; this anyone will learn for himself when he works with it.

In the chemical spectroscope[b] the spectrum is magnified before reaching the eye; the fluids to be examined are generally put into test-tubes, held in a suitable holder before the slit, and the source of illumination is generally a paraffin lamp, although for the complete development of some spectra the oxyhydrogen light may be required. In the murky atmosphere in which I am working, artificial light is almost always required, and I had to wait for two months before sufficient light was got to map the Fraunhofer lines.

For medical purposes I prefer the chemical spectroscope, and without the vernier scale or scale-tube its cost is very trifling.

THE FOUR KINDS OF SPECTRA.[c]

An examination of the light proceeding from various sources has led spectra to be divided into—(1) continuous spectra; (2) the solar and stellar spectra; (3) bright-line spectra; (4) absorption spectra.

Continuous spectra are yielded by all incandescent solids and fluids; gas-light, candle-light, lamp-light, magnesium-light, and the lime-light are examples.

The solar spectrum resembles the last, in that the spectrum is continuous, but differs in the presence of the Fraunhofer lines. These are a number of fine black lines, the absorption bands of elements burning in the sun, which (when the light is sufficiently strong) are seen at right angles to the length of the spectrum; the most prominent, commencing from the extreme red end of the

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[a] For a good account of it, see Suffolk's Spectrum Analysis applied to Microscopical Observation. London : John Browning. 1873. Or Carpenter on the Microscope. Fifth edition.

[b] Roscoe's Lectures on Spectrum Analysis. Third edition. Lockyer—The Spectroscope and its Applications. Nature Series. 1873. Lommel—Optics and Light. International Scientific Series. 1875.

[c] See Deschanel, Lommel, Ganot, Roscoe, &c., *loc. cit.*, for fuller information.

spectrum, are called A, a, B, C, D, E, b, F, G, and H. These lines are the points to which the position of absorption bands is referred.

Bright-line spectra are yielded by the incandescent vapours of metals, and by incandescent gases. The Bunsen flame is sufficient to volatilise some metals, while others require the electric spark. The bright-line spectra of gases are obtained generally by transmitting the electric spark through their attenuated atmosphere, enclosed in a Geissler's tube. A bright-line spectrum is marked by the appearance of bright lines and striæ on a dark back-ground.

As examples of the application of the study of bright-line spectra to medicine, may be mentioned—

(1.) The analysis of the ash of the tissues.[a] A tissue held in the Bunsen flame sufficiently long will sometimes yield the bright lines of sodium and potassium, but it is best to reduce the tissue to an ash; act upon it with water, and then with hydrochloric acid. A piece of platinum foil dipped in the latter solution, and held in the Bunsen flame, will give the bright lines of those metals present which are capable of being volatilised at that temperature. The ash of a Guinea-pig which I cremated yielded the bright lines of sodium, potassium, calcium, and lithium. The ash of the human body[b] is said to yield the bright lines of sodium, potassium, lithium, rubidium, caesium, and calcium.

(2.) Analysis of the ash of calculi[c] when they are small is performed in the same manner.

(3.) The detection of the poisonous metals, when present in small quantity, by means of the spectroscope, has been so fully described by Dr. Emerson Reynolds, in *The Irish Hospital Gazette*,[d] that it is unnecessary to repeat it again.

(4.) Morbid gases may, when present in too small quantity for detection by other means, be recognised by their bright-line spectra.[e] A Geissler's tube provided with a stop-cock, and fitted to an aspirator needle, may be filled with the gas, the tube then exhausted until the pressure is about $\frac{1}{500}$ the pressure of the air, and the electric spark sent through it.

[a] Thudichum (Tenth Report to Privy Council, 1867). On Chemical Identification of Diseases. P. 201.

[b] Thudichum. *Loc. cit.*

[c] Thudichum. *Loc. cit.*

[d] Irish Hospital Gazette. 1873. Pages 6 and 54.

[e] Tenth Report of Medical Officer of Privy Council, 1867.

2 M

(5.) Dr. Bence Jones,[a] in his chapter on the "Chemical Circulation," describes some experiments which were made in order to determine the time that certain salts took to reach various parts of the body; his subjects were Guinea-pigs. The salt—generally one of lithium—was easily detected, by means of the spectroscope, in the different tissues. By an examination of lenses removed from the human body for cataract a certain number of hours after the administration of salts of lithium, this research was extended to man.

Absorption spectra.—When certain coloured solutions are examined spectroscopically, black bands appear in parts of the spectrum—generally in that colour which is complementary to that of the solution—these are called absorption bands. It is not necessary for the coloured substance to be present in solution— *e.g.*, blow-pipe beads coloured with certain salts and some vapours also give absorption bands.

For mapping absorption bands various methods are adopted— *e.g.*, the bright-line micrometer[b] and Sorby's interference apparatus for the microspectroscope,[c] the graduated circle and vernier, and the scale-tube for the chemical spectroscope. In a recent paper I notice the author of it mapped his bands by *comparing* their position with the position of the Fraunhofer lines; such maps are inaccurate. The bright-line micrometer is easily used, but a simple, accurate, and easily applicable method is required for the chemical spectroscope. The following fulfils, I believe, these conditions:—

A NEW METHOD OF MAPPING ABSORPTION BANDS.

The slit of the spectroscope being illuminated by sunlight, it is sufficiently narrowed, and the eye-piece focussed, till the Fraunhofer lines are seen distinctly; a camera-lucida is then slipped over the eye-piece, and a point marked—on a piece of paper placed on the table beneath the camera—just beyond the extreme red, and another beyond the extreme violet. A number of blank spectrum maps are then made of this length, and again brought beneath the camera; the position of the Fraunhofer lines is marked on the top one, and afterwards on all the others. In this way a number of solar maps are made, from which any required number can afterwards be copied.

[a] Lectures on Some of the Applications of Chemistry and Medicine to Pathology and Therapeutics. London. 1867. P. 12, *et seq.*

[b] Suffolk. *Loc. cit.*

[c] Carpenter—Principles of Physiology ; and Crooke's Quarterly Journal of Science. January, 1876.

When an absorption spectrum has to be mapped, a test-tube, containing the solution, illuminated by means of a strong light, is placed before the slit, the right angled reflecting prism is made to cover half the slit, and a Bunsen burner, with a salt of sodium introduced into its flame, is placed so that its light shall fall upon the right-angled prism. On looking into the instrument two spectra are seen, one the absorption spectrum, the other the spectrum of sodium—a yellow line on a dark background. The camera lucida is then slipped over the eye-piece, the maps with the Fraunhofer lines marked on them brought beneath it, and the paper shifted till the bright-yellow sodium line covers the D line on the maps; with a lead pencil the position of the bands and the amount of shading is marked on the maps, care being taken to keep the paper from slipping. The map is afterwards filled in with Indian ink, and then appears as represented in the plate. It must be remembered that the maps have to be turned upside down while being made, otherwise the A line would be on the right-hand side, and the H on the left, in the solar maps, and the absorption bands in the wrong place in the others.

A METHOD OF EXPRESSING THE BREADTH AND POSITION OF ABSORPTION BANDS IN FORMULÆ.[a]

It is a well-known fact to students of spectroscopy that the scales of no two observers are alike, because the dispersion of different instruments differs, so that if a band covers 2 mm. (or any other length) on one map, it may cover 3 mm. on another. The reading of all instruments may, however, be reduced to wave lengths, a tedious process, and one which fails to convey an idea of the breadth of bands. If the relative distances between the Fraunhofer lines in all one-prism chemical spectroscopes and in all microspectroscopes were the same—and I have calculated the following table to show that the differences are so slight as to lead one to believe the lithographer has had something to do with the result—then, if a band cover $\frac{3}{100}$ or $\frac{4}{100}$ of the distance between the Fraunhofer lines B and G on one map, it will cover $\frac{3}{100}$ or $\frac{4}{100}$ of the distance between B and G on another. So that I propose this distance should, *for each instrument*, be divided into 1,000 parts, and all bands expressed in thousandths of the distance between B and G (assumed for conve-

[a] The editor of Nature, in his notice of my method, says :—" The method of wave-lengths is the only one which has a true physical basis." Granted, but my method is simple, useful, and accurate enough for the purpose for which it was intended.

nience $= 1$). The actual division would be difficult, and probably inaccurate, but it may be accomplished in another way with ease— viz., by means of the *diagonal scale.* This scale and the method of constructing it will be found described in any book on mathematical instruments,[*] or an inspection of the accompanying scale, which is adapted to the spectrum maps in the plate, will show how it is made.

The top line of the scale is the distance between B and G, the divisions on it are tenths of that distance, the numbers on the right-hand side are hundredths of the same distance, and denote those parts of the parallel lines which are cut off by the first left-hand diagonal, joining the zero point on the top line with the first division on the bottom one.

It will be readily understood that a diagonal scale has to be made for the maps of every spectroscope.

To express in formulæ the first absorption spectrum in the plate—viz., that of blood, proceed thus—take a pair of compasses and measure the breadth of the first band on the map, and then on the diagonal scale it is found to be ·065, the distance between the bands is found to be ·055, and the breadth of the second band is ·09 ; therefore, placing an asterisk over each band, the diagram may be printed thus—

$$\text{D } \overset{*}{\cdot 065} + \cdot 055 + \overset{*}{\cdot 09} \text{ E,}$$

and in the same way deoxidised hæmoglobin thus—

$$\text{C } \cdot 14 + \overset{*}{\cdot 19} + \cdot 04 \text{ E.}$$

A few more examples will suffice—thus, taking acid hæmatin—a severe test of the method—from the number of absorption bands:—

C $\cdot 03 + \overset{*}{\cdot 07} + \cdot 06$, D $\cdot 015 + \overset{*}{\cdot 02} + \cdot 07 + \overset{*}{\cdot 1}$, E b $\cdot \overset{*}{1}35 + \cdot 015$, F.

Alkaline hæmatin,	-	-	- C $\cdot 075 + \overset{*}{\cdot 1}1 + \cdot 185$, E.
Human bile,	-	-	- C $\cdot 075 + \overset{*}{\cdot 0}85$, D.
The urine band,	-	-	- b $\cdot 06 + \overset{*}{\cdot 1}1 + \cdot 355$, G.
Pettenkofer's test,	-	-	- D$\cdot 18 + \overset{*}{\cdot 1} + \cdot 06 + \overset{*}{\cdot 1} + \cdot 34$, G.

The following table, calculated by means of diagonal scales adapted to the maps of each of the observers whose names are given, shows that the method is universally applicable, and liable

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[*] Heather's Treatise on Mathematical Instruments. Weale's series. 1868.

B C   B   E b   F   G

0   1   2   3   4   5   6   7   8   9

1

2

3

4

5

6

7

8

9
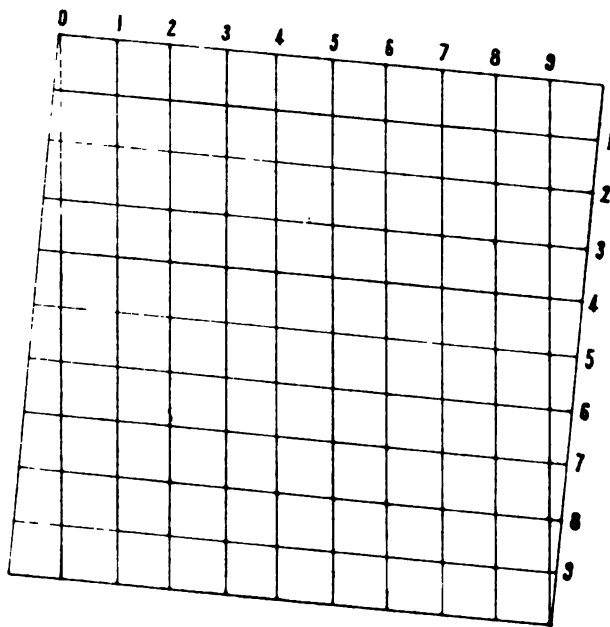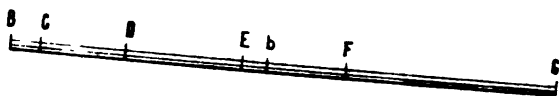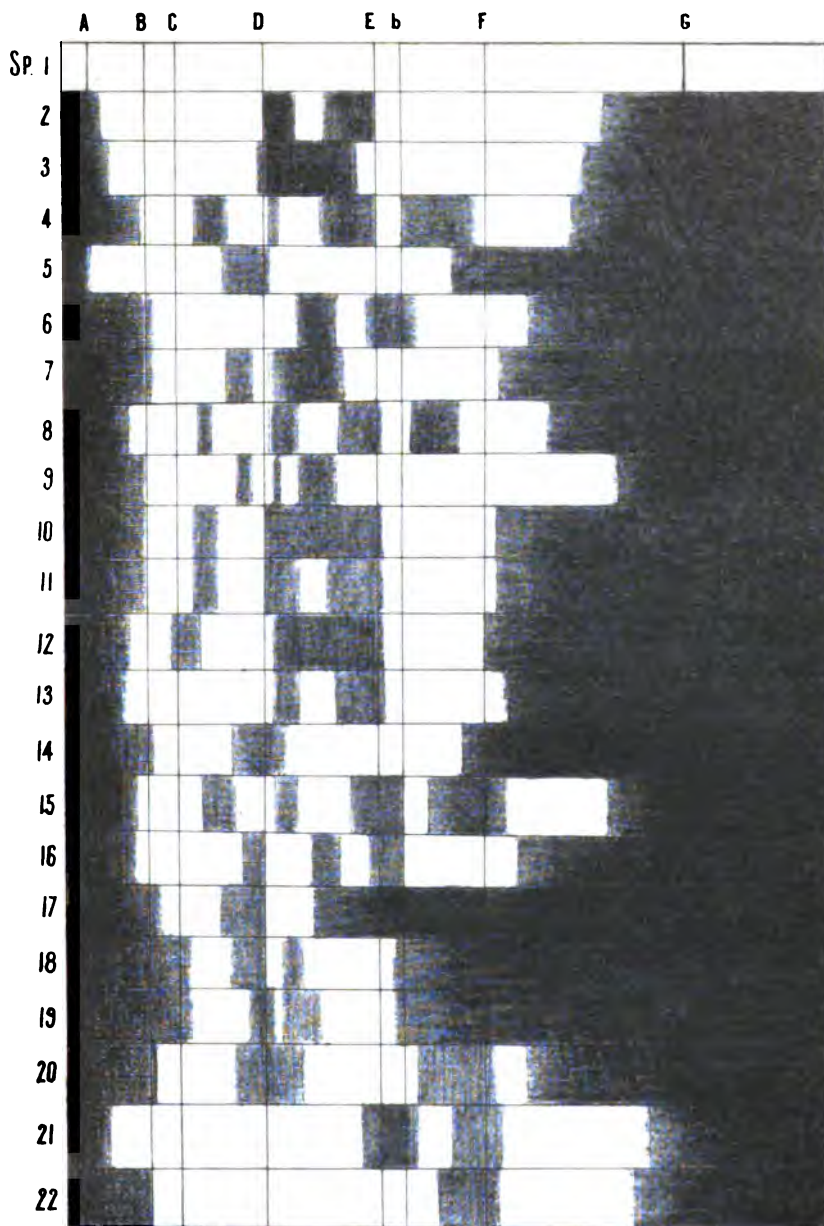
CHAS. A. MAC. MUNN, M.D. AD NAT. DEL.

EXPLANATION OF PLATE.

Sp. 1. Solar spectrum, with Fraunhofer's lines.
   2. O-hæmoglobin.
   3. Deoxidised hæmoglobin.
   4. Acid (four-banded) hæmatin.
   5. Alkaline hæmatin.
   6. Reduced hæmatin.
   7. Sulphate of cruentin.
   8. Alkaline cruentin.
   9. Hydrochloric product of neutral cruentin.
  10. Sulphæmoglobin.
  11.   Do.
  12. Action of hydrocyanic acid on blood.

Sp. 13. The same deoxidised.
  14. Solution of blood treated with solution of bromine.
  15. Blood treated with bromine.
  16. Reduced bromine spectrum.
  17. Human bile.
  18. Ox bile.
  19. Ox bile and nitric acid.
  20. Human bile and nitric acid.
  21. Pettenkofer's test.
  22. The urine band.

to little fallacy; it also shows the accuracy of the camera lucida method of mapping:*—

Table to show the Relative Distances of the Fraunhofer Lines expressed in decimals of the distance between band b *and* G.

Names of Observers	B to C	C to D	D to E	E to b	b to F	F to G
Preyer[b] - -	·06	·155	·21	·04	·15	·385
Thudichum[c] - -	·05	·15	·22	·05	·145	·385
Deschanel[d] - -	·06	·165	·225	·30	·15	·37
Suffolk[e] - -	·06	·155	·22	·045	·14	·38
MacMunn[f] - -	·06	·16	·21	·045	·145	·38
Do.[g] - -	·06	·16	·21	·045	·145	·38

THE ABSORPTION SPECTRA OF BLOOD.

To make this paper complete, all the spectra hitherto described would require mention, but I shall confine myself to those which I have worked out, leaving the rest for future consideration. The blood employed for the different experiments was obtained from men, pigs, oxen, cats, rats, and Guinea-pigs. Pure hæmo-globin, and pure hæmatin were separated from the blood in the usual manner, and the chemically pure solutions studied spectro-scopically; but they differed in no essential respect from the spectra obtained by treatment of defibrinated blood. Accordingly the simplest possible methods of obtaining the spectra are those which will be described.

O-Hæmoglobin.—If a solution containing one part pure defibrin-ated blood to ten of water, be examined with the chemical spectro-scope in a test-tube having a diameter of 18 mm., the red rays only are seen, the rest of the spectrum is dark; at a dilution of one to thirty the red, orange, and green are seen, the last colour being

* These formulæ will be found to differ slightly—when calculated from the plate accompanying this paper—from those given above. This is owing to unavoidable errors in lithographing my maps.—C. A. MacMunn.

[b] Preyer, Die Blutkrystalle. Jena. 1871.
[c] *Loc. cit.*
[d] *Loc. cit.*
[e] *Loc. cit.*
[f] Chemical spectroscope (from camera lucida maps).
[g] Microspectroscope.

separated from the others by a broad dark band extending from above D to below b. On diluting further, this band is found to have been made up of two, one darker and narrower than the other touching D, the other touching E (see sp. 2). At a dilution of about 1 in 2,000, when the test-tube is 15 mm. in diameter, one band only is distinctly visible—viz., that next D. I have found the latter band with the microspectroscope when the dilution reached 1 in 6,000, the cell being 25 mm. deep.

Upon the fact that at a certain degree of dilution the green rays begin to pass, is based Preyer's method for calculating the amount of hæmoglobin in a given quantity of blood.[a]

Reduced Hæmoglobin.—If a solution of blood be treated with Stokes' Solution,[a] an ammoniacal solution of ferrous sulphate, to which enough tartaric acid has been added to prevent precipitation by alkalies; the colour of the blood changes to purple, and one band is seen (shown in sp. 3.) This fluid soon changes, and is not nearly as satisfactory as sulphide of ammonium. Hydrogen and arseniuretted hydrogen also produce this change.[a] On shaking up with air the two-banded spectrum reappears, and on standing the single banded one again takes its place.

The reduction test distinguishes blood from other pigments—*e.g.*, carmine and turacine—a pigment got from the feathers of the Cape lory, both of which resemble it. The physiological bearings of this experiment are elsewhere[d] discussed. The muscles, in cases of death from cholera, have yielded the spectrum of reduced hæmoglobin,[a] which explains the cause of the cramps (suspended oxidation). The reason why venous blood does not give this spectrum is because it always contains a certain amount of oxygen. The blood of asphyxiated animals was said to yield it, but the recent experiments of N. Stroganoff[f] have not supported this opinion.

CO-Hæmoglobin. — By passing carbonic oxide into blood—or coal-gas which contains about 7 per cent. CO—the two bands are shifted slightly towards the violet, and on the addition of reducing agents they are unaffected; hence this test gives *post mortem*

[a] Handbook for Physiological Laboratory, by Klein, Sanderson, Foster, and Brunton. 1873. V. Preyer, *loc. cit.*
[b] Proceed. Roy. Soc. Vol. XIII., 1863-4. P. 355.
[c] Thudichum, *loc. cit.*
[d] Kirke's Physiology, Carpenter, Hermann, &c.
[e] Thudichum, *loc. cit.*
[f] London Medical Record. P. 539. 1876.

evidence in cases of charcoal poisoning. By passing oxygen into the blood before sulphide of ammonium is added, the spectrum of reduced hæmoglobin is seen, just as in healthy blood (Bæblich). Hence, cases of charcoal poisoning have been treated lately in Germany by the inhalation of oxygen.[a]

Acid Hæmatin.—If acetic acid be added to blood, and it is afterwards shaken up with ether, a four-banded spectrum appears (shown in sp. 4). The band just below D is very feeble, and generally difficult to make out; but by using a pretty strong solution, focussing carefully, and *looking sideways*, this band will be seen. The same spectrum may be got by treating blood with alcohol acidulated with sulphuric acid. Preyer[b] calls the same compound hæmatoin, but in the present unsettled state of physiological chemistry it is necessary to be cautious in adopting new names, and there does not seem sufficient reason for altering the old one.[c] Sometimes two feeble bands appear instead of the single one next D.

Alkaline Hæmatin.—The simple addition of an alkali to blood will *not* change its spectrum; thus a large quantity of ammonia may be added without any apparent effect (which probably explains why the injection of ammonia into the veins is not productive of harm); but if a solution of carbonate of potash in alcohol is added, the spectrum as well as the colour of the solution changes, and on spectroscopic examination sp. 5 is seen. Hæmatin prepared from blood, according to the directions in Ralfe's Physiological Chemistry,[d] was found to be impure, but prepared according to Dr. Thudichum's directions,[e] it gave all the spectra which he describes. The spectrum of pure alkaline hæmatin is exactly the same as that got by the above method.

Reduced Hæmatin.—By adding sulphide of ammonium to the solution of alkaline hæmatin, two bands appear nearer the violet than the blood-bands (sp. 6), and by shaking up with air they disappear to be replaced by the original band.

Sulphate of Cruentin.[f]—By boiling defibrinated blood with

[a] Nature. Vol. XV., p. 362.
[b] *Loc. cit.*
[c] V. Thudichum, *loc. cit.*, p. 232; *cf.* with Handbook for Physiological Laboratory on Hæmatin, and with Preyer, *loc. cit.*
[d] Outlines of Physiological Chemistry. 1873.
[e] Thudichum. Chemical Physiology. 1872. P. 128.
[f] Thudichum, *loc. cit.* This appears to be identical with the iron-free-hæmatin of Hoppe-Seyler.

strong sulphuric acid, adding water and filtering, a brownish pre-cipitate is obtained in the filter, which contains impure cruentin. If some of the mass be dissolved in sulphuric acid, a fine ruby red solution is obtained, which gives two absorption bands—V. sp. 7 (a third is sometimes seen, but it very soon disappears).

Neutral Cruentin.—The mass on the filter in the last experi-ment is washed to neutrality and dried: a little dissolved in ether or chloroform gives the same spectrum as the next.

Alkaline Cruentin, which is obtained by dissolving some of the neutral dried precipitate of the last experiment but one in alcohol and ammonia—V. sp. 8.

Reduced Cruentin[a].—I have never yet succeeded in getting the spectrum of reduced cruentin, which is got by deoxidising the last with Stokes' fluid, or with NH_4 HS. It is said to give three bands—one at C, one between D and E, and one covering E and b.

Hydrochloric Product of Neutral Cruentin.—By adding HCl to the chloroformic solution of neutral cruentin, and shaking and warming, sp. 9 was obtained.

Sulphæmoglobin.[b]—On passing sulphuretted hydrogen for a suffi-cient time through a solution of blood, the colour of the solution changed from scarlet to a lilac tint, and sp. 10 was then seen; on shaking with air, sp. 11 appeared; and on standing, sp. 10. This compound sometimes forms in putrid blood. If a reducing agent be added to blood before H_2S is passed through it (Preyer), this compound is not formed. This, as Dr. Purser[c] remarks, explains why water containing H_2S can be drunk, or the latter injected into the veins without danger to life, while the danger of breathing it is very great.

Action of Hydrocyanic Acid.—Hydrocyanic acid acts differently, according to the quantity added; sp. 12 is the result of about thirty experiments. On comparing this with Preyer's map of his "Cyanwasserstoff-sauerstoff hämoglobin"[d] it will be noticed that my map contains an additional band in red at C. The broad band somewhat resembles the band of reduced hæmoglobin, but is lower in the spectrum, and is more shaded towards the b line. On adding to this solution sulphide of ammonium, sp. 13 is obtained; on exposing to the air the original, spectrum 12 is reproduced.

[a] Thudichum, *loc. cit.*

[b] Lankester. Journal of Anatomy and Physiology, 1869. Also Dr. Purser, Irish Hospital Gazette, Dec. 15, 1873. P. 375.

[c] *Loc. cit.*

[d] Die Blutkrystalle. Taf. ii. (Sp.) 12.

The blood of animals poisoned by HCN does not give this spectrum. Athough this matter was settled before, I made some experiments for my own satisfaction. In one, 4 cc. of Scheele's prussic acid were given to a cat; it died at once; the heart was exposed without delay; its apex snipped off, and the blood received in a small beaker, but on examination the blood bands only were seen. Four other experiments gave the same result.

Action of Bromine.—The action of bromine on blood has not, I believe, been hitherto described. It appears to act differently, according as pure bromine or diluted bromine is used.

(1.) If 3 drops or so of pure bromine be added to half a test-tube full of water, and a few drops of this solution added to a solution of blood, a brownish fluid is obtained, giving one absorption band, covering D—sp. 14.

(2.) If pure bromine be added to undiluted blood, and water afterwards added, a reddish-brown solution is obtained, giving four absorption-bands, which are shown in sp. 15. On adding sulphide of ammonium to either of these solutions, sp. 16 is obtained. Thus bromine acts like acids and alkalies respectively; the spectrum of diluted bromine and blood is like the spectrum of alkaline hæmatin, while the spectrum of pure bromine and blood is like that of acid hæmatin.

Action of Iodine.—Iodine, in aqueous or alcoholic solution, when added to blood, gives four absorption bands which differ in their breadth from those of the bromine spectrum; but I reserve the description of this spectrum, as well as others of pathological interest, for a future communication.

ABSORPTION SPECTRA OF BILE.

Ox-bile.—Fresh, green ox-bile gives the spectrum of sp. 18— a feeble band above D, and a darker one between D and E. When the bile decomposes it turns of a brownish colour and the same spectrum is seen. Sheep-bile gives the same spectrum.

Ox-bile treated with Nitric Acid.—On treating ox-bile with nitric acid, and at once adding boiling alcohol, or filtering quickly, sp. 19 is seen. In both cases the spectrum is shaded on each side of the bands, up to C on one side and E on the other.

Human-bile.—Bile procured from the gall-bladder of patients who died at the Wolverhampton hospital, when diluted with water,

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Dalton, New York Medical Journal, 1875, has described some bile spectra, but unfortunately I have not read his paper.

and *examined in a sufficiently deep layer*, gave one band very like that of alkaline hæmatin—sp. 17; on adding alcohol, and filtering, this band is much better marked.

Human-bile treated with Nitric Acid.—When an aqueous solution was treated with nitric acid and quickly filtered, or boiling alcohol added, to save time, sp. 20 appeared. Hydrochloric acid had the same effect, and on diluting these solutions a point is reached at which half the shading below *D* disappears to leave the half above it; on diluting still further the *band at F only is left*— this band is almost identical with the urine band in position.

Bilirubin, [a] *bilifuscin*, and *biliverdin*, were separated from bile and from human gall-stones in the usual manner, but gave no spectra.

Bilicyanin, [b] got by acting on biliverdin with nitric acid, gave a band close to F.

Cholocyanin. [c]—An ammoniacal solution of bilirubin was treated with nitric acid till a precipitate of a deep-blue colour formed; it was then filtered and dissolved in alcohol; it gave a very feeble band just above D.

Pettenkofer's Test.—The discrepancies regarding the spectrum of Pettenkofer's test [d] in the statements of different authors, led me to make this experiment. Human bile was mixed with much alcohol, let stand, and filtered, the filtrate treated with animal charcoal till a filtered portion came away colourless; the whole was then filtered again, and sugar and sulphuric acid added. In the last filtrate the spectrum was rather difficult to make out, but by using a strong light, and examining in a thin layer, sp. 21 was seen. An aqueous solution of the bile acids gave the same spectrum.

That bile pigment is derived from hæmatin, is very probable, seeing the close resemblance there is between the spectra of alkaline hæmatin and bile. How is it that of all the bile pigments hitherto isolated, none have given the spectrum of bile itself ?

[a] See Fownes' Chemistry, 1873; also Tenth Report of Medical Officer of Privy Council, 1867, for a historical retrospect of bile pigments by Thudichum. Also Handbook to Physiological Laboratory, *loc. cit.*, and Hermann's Physiology, translated by Gamgee, 1875, p. 28, *et seq.*

[b] Heynsius and Campbell.

[c] Thudichum. *Loc. cit.*

[d] According to the authors of the Physiological Handbook there are five bands; Thudichum figures one and describes two, while Dalton figures two.

ABSORPTION SPECTRA OF URINE.

The Normal Urine Band.—Every specimen of urine, when examined in a sufficiently deep layer, gave a band low down in the spectrum, at the F line, sp. 22. In high-coloured urine it is much better marked.

By the addition of liq. sodæ the band is replaced by a narrower band high up between b and F* (KHO causes it to disappear, but HNO_3 again restores and intensifies it).

It has been said that a band identical in position may be got by treating dog's bile with dilute hydrochloric acid and filtering, and that the latter acts in a similar manner to the urine band when treated with liq. sodæ.[b] On comparing the position of the band at F, in sp. 20, with that in sp. 22, it will be seen how nearly identical they are in position, and I have mentioned under the bile spectra that the solution from which No. 20 was mapped could be diluted till only the band at F was left. I think it is therefore highly probable that the pigment giving the urine band is derived from bile pigment, and it is a question if the real colouring matter of urine has not yet to be isolated. Recent researches have called the experiments of Schunk,[c] relating to the presence of indican in the urine, in question, and, according to the authors of a recent paper[d] on the present state of physiological chemistry, its presence in urine is more than doubtful.

Bile Pigment in Urine.—The usual test for bile pigment in the urine is Gmelin's, or the nitric acid test, the fallacies attending which have been often pointed out.[e] In most cases where jaundiced urine is treated with nitric acid, it gives the spectrum which bile itself gives when treated in the same way. In some cases rapid filtration is necessary after the addition of the acid; in others the urine may be shaken up with chloroform, the former decanted off after being allowed to stand for some time, and the chloroformic solution, after being allowed to evaporate to a sufficient degree of concentration, treated with nitric acid; in this way sp. 20 was

[a] *Cf.* Handbook for P. Laboratory. *Loc. cit.*

[b] *Cf.* Handbook for P. Laboratory. *Loc. cit.*

[c] Proc. Royal Society, 1867.

[d] Kingzett and Hake on Physiology and its Chemistry at Home and Abroad, in Crooke's Journal of Science, Jan., 1877.

[e] See Dr. Walter G. Smith on "Some New Tests for Bile Pigment." Dub. Jour. Med. Sci., Dec., 1876. P. 449.

obtained in every instance. I find the addition of caustic potash, previous to that of nitric acid, seems to intensify the effect.

Bile Acids in Urine.—·065 gram. of bile salts (taurocholate and glycocholate of soda) were added to nine cubic centimetres of healthy human urine, and Pettenkofer's test applied; sp. 21 was obtained. To some urine containing bile salts some crude bile was added, and the test repeated. The band covering E, b, was well marked, but the rest of the spectrum was shaded.

Blood in Urine.—If present in solution, it gives the spectrum of O-hæmoglobin. A blood-clot (the nature of which is doubtful) may be treated with alcohol, acidulated with sulphuric acid, when sp. 4 will be obtained.

Since writing the above, I found, in two specimens of urine from children suffering from acute desquamative nephritis, an additional band in red, besides the normal blood-bands; this was found to be the band of met-hæmoglobin.

ART. XX.—*The Influence of Chemical Constitution on Physiological Activity.*[*] By J. EMERSON REYNOLDS, M.D., F.C.S.; Professor of Chemistry, University of Dublin.

AT the close of my last lecture it was stated that additional evidence would be brought forward in support of our second proposition, which asserts "that groups of bodies exist which contain the same elements in different proportions, and are similar in chemical structure, but which differ materially in degree of physiological activity; and that the observed alteration in action can be explained on chemical grounds." Having already given in evidence the exceedingly interesting experiments of Professor Gamgee on the sodic phosphates, I will now refer to some corresponding experiments on vanadates made by Messrs. Priestly and Larmuth.

The admirable researches of Roscoe have recently made us acquainted with the true nature and relations of the compounds of the rare metal vanadium. It has been long known that vanadium yields several acids analogous to the phosphoric acids dealt with in the last lecture. Roscoe has shown that a series of sodic vanadates exist corresponding to the sodic phosphates already

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[*] The substance of the second of two Lectures delivered before the King and Queen's College of Physicians in Ireland, on January 22nd and 29th, 1877.

referred to. Thus we have ortho-, pyro-, and meta-vanadates, which are the strict analogues of the ortho-, pyro-, and meta-phosphates, and their formulæ may be written in a similar manner, the pentad element phosphorus being replaced by vanadium ($=V$), which is also a pentad. In a very elaborate memoir Priestly has given the results of his researches on the physiological action of the vanadic salts, and has shown that they exert a strong irritant action on the gastro-intestinal mucous membrane, while they are also remarkably active as poisons of the spinal cord, the medulla oblongata, and the heart. The active vanadium compounds paralyse the vaso-motor centres; they do not affect the vagus as a cardiac inhibitory nerve, but exert some influence upon the intrinsic nervous mechanism of the heart.

Larmuth has compared the action of the three sodic vanadates, and shown that the ortho-vanadate exerts distinct poisonous action, though less than the pyro- and meta-vanadates; he has further proved that the pyro-vanadate is most energetic as a poison, while the meta-vanadate is intermediate in activity between the two extremes. The order of activity is, therefore, the same in the group of vanadates as in that of the phosphates. I may add that the analogous arsenates agree to some extent with the vanadates in being all poisonous, and in exhibiting differences in degree of activity.

In the cases just mentioned the observed differences in extent of action are probably to be explained by the aid of the considerations which guided us in dealing with the group of phosphates, for it is evident that the degree of activity is in the inverse ratio to the degree of satisfaction of the triad nucleus ($P\,O'''$, $V\,O'''$, or $As\,O'''$) with basic material, and seems to be dependent upon that condition. It will be noted, however, that the ortho-vanadate and the corresponding arsenate differ from the ortho-phosphate in exerting well-marked physiological action. It may be that this marked distinction is to be connected with the facility with which ortho-vanadates and arsenates can be reduced to lower states of oxidation by means which are wholly insufficient to effect the reduction of phosphates. The product of such reduction is necessarily an unsaturated compound, and is, therefore, capable of exerting physiological action. Rabuteau has shown that certain bodies easily suffer reduction or deoxidation during absorption— thus, selenious acid ($H_2\,Se\,O_3$) and tellurous acid ($H_2\,Te\,O_3$), or selenites and tellurites, lose oxygen in transit, and are reduced to

selenuretted hydrogen (H_2 Se) and telluretted hydrogen (H_2 Te) respectively. But this reducing action is not very powerful, as shown by the fact that sulphurous acid (H_2 S O_3) and sulphites do not suffer reduction, and in fact are oxidised into sulphuric acid and sulphates; and even the salt commonly called "hypo-sulphite of sodium" (Na_2 S O_3 S, or sodic thio-sulphate) resists reduction like the sulphites and the sulphates. Vanadates and arsenates seem to belong to the class of bodies which suffer partial reduction during absorption, or when in contact with insufficiently oxidised blood, while the phosphates do not suffer that change, so far as we know.

In view of these facts we have reason to think that the action of the vanadates and arsenates is complicated by secondary changes, whose effect is quite intelligible in altering physiological activity. It is, then, in the group of phosphates that we recognise most distinctly the influence of the degree of saturation or satisfaction of the grouping element upon the physiological activity of the compound.

If the reduction theory which we have just suggested be true, a saturated vanadium or arsenic compound which happened to be *difficult of reduction* ought, even if easily dissolved and absorbed, to be physiologically inactive. I cannot bring forward evidence affecting vanadium compounds as yet, but the researches of Bunsen have made us acquainted with a very remarkable compound of the much more energetic metal, arsenic, which fulfils our expectations completely. The substance referred to is called kakodylic acid, and is related to one of the arsenious acids in the following way:—

Arsenious acid.
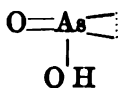
Kakodylic acid.
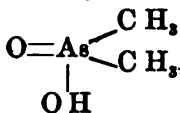
Arsenious acid we all know to be highly poisonous, but the easily soluble kakodylic acid, containing 54 per cent. of metallic arsenic, is inert.

Passing from this group of bodies, let me now briefly refer to the alcohols. We are acquainted with a large number of chemical compounds which are classed by chemists as alcohols, because they resemble more or less in chemical relations the body to which the term "alcohol" is commonly restricted—thus the syrupy liquid

glycerine belongs to one division of the alcohols, and the white crystalline substance mannite, extracted from manna, belongs to another section. The bodies with whose action I now propose to deal differ from those just referred to in being very closely related to the chief member of the group—the common alcohol of wine, as well in their chemical relations as in their action upon the animal organism. Their names and formulæ are as follow:—

MONATOMIC ALCOHOLS.

Methyl alcohol (wood spirit),
$$\begin{array}{c} -H \\ | \\ C\,H_2 \\ | \\ -O\,H \end{array}$$

Ethyl alcohol (spirit of wine),
$$\begin{array}{c} -C\,H_3 \\ | \\ C\,H_2 \\ | \\ -O\,H \end{array}$$

Propyl alcohol,
$$\begin{array}{c} -C\,H_2\,C\,H_3 \\ | \\ C\,H_2 \\ | \\ -O\,H \end{array}$$

Butyl alcohol,
$$\begin{array}{c} -C\,H_2\,(C\,H_3)_2 \\ | \\ C\,H_2 \\ | \\ -O\,H \end{array}$$

Amyl alcohol,
$$\begin{array}{c} -C\,H_2\,C\,H\,(C\,H_3)_2 \\ | \\ C\,H_2 \\ | \\ -O\,H \end{array}$$

All these alcohols, except the first of the series, are produced during the fermentation of grain spirit; but the three last-named occur in very small quantity, and are found in the poisonous "fusel oil" of new whiskey. The poisonous action of "fusel oil" is chiefly due to the presence of these higher alcohols.

These alcohols, or isomeric modifications of them, have been

made the subject of numerous and interesting researches, con-
ducted with a view to the comparison of their physiological action.
The experiments which are of most direct interest to us in the
present connexion are those of Messrs. Dujardin-Beaumetz and
Audigé, and of Dr. B. W. Richardson, of London. As the
researches of Dr. Richardson on this subject are, no doubt, familiar
to my hearers, I shall confine my remarks to the work of the
French investigators.

Messrs. Beaumetz and Audigé have shown that the alcohols
above named act most energetically when introduced into the
stomach of an animal. By means of a series of experiments, which
seem to have been conducted with care, they have arrived at the
conclusion that the lethal dose of each alcohol per pound weight of
animal is approximately—

> 42 grains of ethylic alcohol.
> 33　„　propylic　„
> 13　„　butylic　„
> 5　„　amylic　„

It would appear, then, that the alcohols which have the most
complex molecules are most energetic. Beaumetz has, however,
still more recently shown that methyl alcohol (the lowest term of
the series of compounds) is more poisonous than the alcohol of
wine. With this important exception, the statement seems to be
generally true.

Now, all these bodies are represented in the diagram as saturated
compounds, and they are so; but they all contain one atom of
hydrogen which is easily displaced by various radicles, so that
under favourable conditions they are capable of acting as if they
were unsatisfied though saturated. It is probable that the phy-
siological activity exhibited by the lower members of the series—
with the exception of methylic alcohol—is due to this cause, but it
cannot be the sole cause, else we should expect all the members of
the group to act with nearly equal energy; and Beaumetz and
Audigé's results, cited above, as well as Richardson's, prove that
the alcohols are not equally active. It is probable that the members
of the series of more complex constitution not only act as I have
just mentioned, but partially break up in the organism under the
influence of oxygen or other agents, and afford as products of their
decomposition more simple but unsaturated bodies, which then exert
the marked effects of compounds of that class. As the number of

carbon atoms in the molecule of the alcohol increases, the number of unsaturated bodies which a given weight can yield also increases; and it is, at any rate, a remarkable coincidence that there is a corresponding gain in physiological activity.

It is now necessary to turn for a moment to the first member of the group of alcohols—methylic alcohol—a body which is, according to Beaumetz, more poisonous weight for weight than the more complex alcohol of wine. If we admit the fact, the explanation of it is probably to be found in the circumstance that methyl alcohol can be much more readily broken up by oxidation than ethyl alcohol; and one of its products of oxidation is formic acid—a substance which, though seemingly inactive when directly introduced into the system, appears, according to the researches of Byasson, to be capable of producing very marked effects when formed within the organism, and so presented, as it were, in the nascent state.

Before leaving the group of alcohols, I may add that we are acquainted with isomeric propylic, butylic, and amylic alcohols, whose physiological effects yet require careful study.

If time permitted I might refer to other and highly interesting researches in support of our proposition—notably to those of Cahours and Jolyet on the action of substituted anilines, and of Dewar and M'Kendrick on the action of the pyridine bases, while the study of the action of chloral hydrate and its homologues, of formic and oxalic acids, and of many other bodies, could be shown to afford additional evidence—but I trust enough has been said to satisfy you that a strong experimental foundation exists for the statements made.

Before passing to the consideration of our next proposition, I may be permitted to digress so far as to point out that we can alone correctly compare the physiological activity of one element with that of another when we study compounds of similar chemical constitution. Thus sodic nitrate (meta-nitrate), meta-phosphate, meta-vanadate, and probably meta-arsenate, accord in composition with the general formula, $Na\,R\,O_3$, and are similarly constituted bodies. If otherwise suitable, their comparative study ought to afford us data for the determination of the relative physiological activity of the elements, nitrogen, phosphorus, vanadium, and arsenic, when in that particular form of combination. So far as experiments of this kind have been made with the four elements just named, it appears that nitrogen is lowest in the scale of activity; then follow phosphorus and vanadium, while the most energetic of all is

2 N

arsenic.   If we now compare the atomic weights of these elements, we find that the physiological activity increases with the atomic weight.   Thus, the

Atomic weight of	nitrogen is	-	14·0
„	„ phosphorus	-	31·0
„	„ vanadium	-	51·2
„	„ arsenic	-	75·0

Considerations of this order led Rabuteau to state generally that the physiological activity of an element is directly proportional to its atomic weight. I need only to mention here, however, that "Rabuteau's law" is but partially true, even when we compare similarly constituted groups of active bodies, for when we thus compare some corresponding compounds—chlorine, bromine, and iodine—we find that chlorine (at. wt. 35·5) is more active than bromine (at. wt. 80), and bromine than iodine (at. wt. 127).   In fact, if we substituted for "Rabuteau's law" the statement that the physiological activity of an element is in the inverse ratio to its attraction for oxygen, we should enunciate a "law" which seems to be, on the whole, open to fewer objections than Rabuteau's, but which is, nevertheless, governed by the question of constitution.

In this connexion I might also refer to the interesting experiments of Blake, who sought to connect the physiological action with physical structure, and who asserts that isomorphous salts produce similar effects on the animal organism; and to the experiments of Dr. B. Wills Richardson on the comparative effects of the ordinary urea and the sulpho-urea which I discovered some years ago; but I must leave the detailed consideration of these cross-comparisons for another and more convenient time.

THIRD PROPOSITION.

Our third proposition asserts "*that bodies which are unlike in composition, but which agree in being either actually or constructively unsaturated, and are alike in certain marked chemical relations, often agree in physiological action.*"

Few bodies differ from each other more than the element phosphorus from the well-known compound called pyrogallic acid, or pyrogallol; yet Personne asserts that they act in the same way in the animal organism, because they agree in one strongly-marked chemical character—namely, intense avidity for oxygen.

Personne believes that absorbed phosphorus poisons by abstracting oxygen from the blood, and that the death of an animal poisoned

by phosphorus is really due to asphyxia. If this be a true explanation, argued M. Personne, any other body capable of absorbing oxygen with equal energy from the blood ought to resemble phosphorus in its action on the animal organism. Pyrogallic acid is such a body, especially when present in a slightly alkaline liquid; consequently Personne injected 60 grains of the pyrogallic acid, dissolved in a large quantity of water, into the stomach of a dog. Symptoms of poisoning were quickly developed, and closely resembled those caused by phosphorus. The animal died, and the morbid changes observed were precisely similar to those well known to be produced by phosphorus. A second dog received a dose of 30 grains of the pyrogallic acid, and the same train of symptoms and morbid appearances were observed.

Ordinary waxy phosphorus is, as you are aware, a body which possesses most energetic properties, and is unsaturated, as all elements are. It is, moreover, one of the very few chemical elements which admits of being absorbed to any sensible extent in the elemental condition, owing to its solubility in, or miscibility with, fatty matter. Pyrogallol, or pyrogallic acid, has the composition represented by the formula $C_6 H_3 (O H)_3$, and the carbon atoms within its molecule are very far from having their "bonds" completely saturated by hydrogen and hydroxyl, as there are at least six "bonds" latent in the group, and some of these appear suddenly to become active when the compound is mixed with an alkali, although in the absence of alkali the pyrogallol does not exhibit any very energetic properties. The extreme avidity with which ordinary gaseous oxygen is absorbed by the alkaline pyrogallic solution seems to be due to the breaking up of the pyrogallol, under the influence of the alkali, into a number of unsaturated groups, which are as energetic in some at least of their chemical relations as phosphorus, for they even more readily combine with free oxygen. Personne's view—that any pyrogallol introduced into the slightly alkaline blood acts in a similar manner—was, therefore, not an unreasonable one.

The molecule of pentad phosphorus is chemically divisible into four parts, or atoms, unlike the free molecules of most other elements, which consist of two atoms only. The probable structure of the phosphorus molecule on this view is represented thus:—

$$
\begin{array}{c}
\text{P}\!=\!\text{P} \\
||| \quad ||| \\
\text{P}\!=\!\text{P}
\end{array}
$$

The probable structure of the molecule of pyrogallol may be thus indicated:—

$$
\begin{array}{ccc}
\text{O H} & & \text{H} \\
| & & | \\
\text{C} & \!\!\!\!-\!\!\!\!- & \text{C} \\
\end{array}
$$
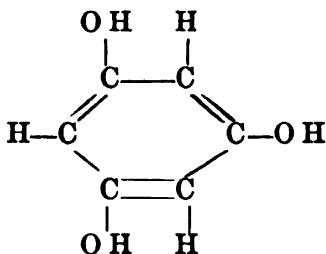
This mode of writing the formula enables us easily to understand how the body can break up symmetrically under the influence of alkali into unsaturated groups. While ready to accept Personne's experiments I am not disposed to follow him to his conclusion that phosphorus poisons by abstraction of oxygen, because (1) the symptoms of phosphorus poisoning are not those common to asphyxiants; and (2) Curie and Vigier have shown that the quantity of phosphorus required to poison a rabbit weighing 3 kilogrammes (6·6 lbs.) is only sufficient to absorb one centigramme (·154 grain) of oxygen. The facts only warrant the statement contained in our proposition, as phosphorus and pyrogallol seem to agree in physiological action as well as in power of absorbing oxygen, though they are wholly unlike in composition; and it is highly probable that this resemblance in activity is due to the actually unsaturated condition of phosphorus, and to the constructively unsaturated condition of pyrogallol.

FOURTH PROPOSITION.

I have now to assert that "*the result of chemical addition to unsaturated molecules is either to destroy or to materially modify their physiological action.*"

In all the cases hitherto dealt with we have been able to connect physiological activity with the actual or constructive unsaturation of the molecule of the body considered. We now have to determine the effect of chemical addition to the unsaturated or unsatisfied groups, upon the physiological activity of the body experimented with, and to note how far this is altered. In all such experiments we have to take care that the elements or groups added shall produce a new compound which will not easily decompose in the animal organism.

Carbonic oxide is a good example of an unsaturated compound whose special action is destroyed by the addition of an atom of oxygen, so as to form the familiar carbon dioxide or "carbonic acid." In carbonic oxide the tetrad carbon atom has but two of its four "bonds" satisfied by diad oxygen; in the higher oxide all the bonds are engaged.

Carbonic oxide.   "Carbonic acid."

:⊃C◇⊃O   O◇C◇O

The unsaturated carbonic oxide we know readily unites with the hæmoglobin of the blood to form Preyer's carbonic oxide-hæmoglobin—a body which, when once formed, very slowly undergoes change, even in presence of oxygen, and therefore neutralises, for the time, a certain quantity of the great oxygen carrier of the blood, and interferes to a corresponding extent with the vital processes. If the amount of carbonic oxide compound formed is large, the absorption of oxygen by the blood can take place to a very limited extent, and death ensues. On the other hand, "carbonic acid"—the saturated compound—does not combine with hæmoglobin, and, when inhaled by an animal, prevents oxidation chiefly by excluding air, or by so diluting it as to render the oxygen supply insufficient for the amount of blood pumped through the lungs, and consequently favours the accumulation of effete products in the system.

The poisonous action of carbonic oxide is direct and consequent on the unsaturated condition of its molecule. The poisonous action of "carbonic acid" seems to be, in great part, traceable to a physical cause. The result of the chemical addition in this instance is, at any rate, to materially modify physiological action.

Bunsen's remarkable experiments on the kakodyl compounds, already alluded to (p. 530), have shown that chemical addition of a certain kind ($2 \ C \ H_3$) to an arsenious acid destroys the power of that well-known poison.

Again, Rabuteau states that cyanates are not poisonous. If we regard potassic cyanide as the analogue of a carbamine, as I pointed out in the last lecture, its relation to a cyanate may be thus shown :—

Potassic cyanide.   Potassic cyanate.

⊃N=C◇:   ⊃N=C⊃O
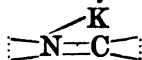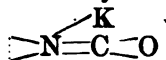
Thus by the addition of an atom of oxygen to potassic cyanide the salt loses its extraordinary physiological activity. ·

I may remind you of the experiments which were dwelt upon in the last lecture, and which prove that sodic ortho-phosphate is inert, though the pyro- and meta-phosphates exhibit considerable physiological activity. I then pointed out that the two latter salts can easily be converted into the inactive ortho-phosphate by the addition of basic oxide; hence chemical addition in these as in the other cases mentioned destroys the physiological activity of the body so treated.

In most of the cases just cited in support of our proposition there is a seemingly total destruction of the physiological activity of each body consequent upon the complete satisfaction of the tendency to accumulate matter within the molecule. I shall now turn to the work of the distinguished pioneers of this branch of inquiry—Drs. Crum-Brown and Frazer, whose results place beyond question the fact that chemical addition to unsaturated compounds— notably to the vegetable alkaloids—can materially modify, but not necessarily destroy their physiological activity; but it must be borne in mind that in all those cases in which a certain residual effect has been observed we are unable to assert that all the " bonds " of the body treated have been fully satisfied. In fact, the argument from analogy rather leads us to the conclusion that the satisfaction is but partial.

The two well-known alkaloids, strychnia and brucia, are un-saturated bodies, for they both easily unite with methyl iodide or sulphate to form seemingly saturated compounds whose relation to the parent base may be thus represented in the cases of the iodides :—
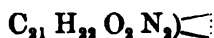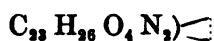
Strychnia.	Iodide of methyl-strychnium.
$C_{21} H_{22} O_2 N_2 \langle$	$C_{21} H_{22} O_2 N_2 \langle{}^{C\,H_3}_{I}$
Brucia.	Iodide of methyl-brucium.
$C_{23} H_{26} O_4 N_2 \langle$	$C_{23} H_{26} O_4 N_2 \langle{}^{C\,H_3}_{I}$
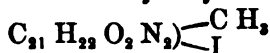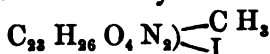
The methylated derivatives of these and other alkaloids are not only easily prepared, but they possess the great advantage of being difficult of decomposition by the agents they are likely to meet in the animal organism. The comparative physiological experiments made by Drs. Crum-Brown and Frazer with strychnia and its methylated derivatives, conducted, as they were, with consummate skill, have afforded the remarkable results which I can best state in the words of the distinguished investigators :—

" These experiments clearly prove that the methyl derivatives of strychnia possess a very different action from strychnia itself. In none of our experiments, not even in the fatal cases, were the symptoms those of strychnia-poisoning; no starts nor spasms occurred, nor did stimulation give evidence of the slightest increase of reflex excitability.

"In fact, a condition exactly the reverse of that produced by strychnia was produced by these compounds. In place of violent spasmodic contractions and muscular rigidity, the appearances were those of paralysis, with a perfectly flaccid condition of the muscles. The limbs of the animal first yielded, its head gradually sank until it rested on the table; by-and-by it lay in a perfectly relaxed condition, and when death occurred it was due to stoppage of the respiratory movements. In the autopsies further evidence was obtained to distinguish the effects of the methyl-strychnium compounds from those of strychnia. The heart was found acting with nearly its normal rapidity; the spinal motor nerves were either paralysed or nearly so; and in place of the almost immediate occurrence of *rigor mortis* that follows the action of strychnia, the muscles continued flaccid, contractile, and alkaline for many hours.

"These symptoms are sufficient to suggest a close resemblance between the action of the methyl derivatives of strychnia and that of curare (*wourali*)—a well known and elaborately studied poison. In a recent publication Professor Schroff, of Vienna, has indicated a resemblance of this kind between the nitrate of methyl-strychnium and curare. Both substances undoubtedly produce a condition of general paralysis; but the special characteristic of curare-poisoning is that this paralysis is the result of an impairment or destruction of the function of the peripheral terminations (end-organs) of the motor nerves. It is impossible to demonstrate such an action without undertaking experiments of a special character. We accordingly extended our research for the purpose of examining this question.

"The sciatic artery and vein were tied at the knee of a frog, and one-tenth of a grain of sulphate of methyl-strychnium, dissolved in distilled water, was injected under the skin of the back. Eight minutes afterwards the frog was lying in a perfectly flaccid state, and in ten minutes irritation of any portion of the skin produced energetic movements of the tied limb *below the points of ligature*, but nowhere else. The sciatic nerve of the untied limb was now exposed, and, on stimulating it with a weak interrupted galvanic current, movements occurred in the tied limb only; not the slightest

effect occurred in any part to which the poison had access. At the same time the muscles were everywhere active, and freely contracted when directly stimulated. The sciatic nerve was then exposed in the tied limb *above the points of ligature*, and on stimulating it energetic movements occurred below the knee of that limb, and there only. The heart was at this time acting at the rate of fifty per minute.

" This experiment was repeated with one grain of iodide of methyl-strychnium, and the same general results were obtained. The evidence that was thus acquired in favour of an action on the peripheral terminations of the motor nerves was strengthened by a modification of this method of experiment.

" The right gastrocnemius muscle of a frog was carefully dissected from its connexions, excepting that its origin and insertion, and the nerve-fibres entering it, were untouched, and that all its blood-vessels were ligatured. One-tenth of a grain of sulphate of methyl-strychnium, dissolved in five minims of distilled water, was then injected under the skin of the back. Twenty minutes afterwards, the animal being in a perfectly relaxed and motionless condition, the two sciatic nerves were exposed. Galvanism of the left produced no movements of the left limb, while galvanism of the right produced energetic movements of the right limb, which were seen to be due solely to contraction of the right gastrocnemius muscle, the other muscle remaining motionless. At the same time direct stimulation by galvanism caused contractions as freely in the poisoned muscles as in the non-poisoned right gastrocnemius.

" In an experiment, in which iodide of methyl-strychnium was substituted for sulphate, the effects were the same. We have, therefore, demonstrated that the methyl-strychnium derivatives produce paralysis and death by destroying the function of the motor nerve end-organs, and that their mode of action is, therefore, identical with that of curare. This conclusion is an extremely curious and interesting one. It is difficult to imagine a more decided modification in the action of any substance than has been produced by the addition of iodide or sulphate of methyl to strychnia. The striking characteristic of strychnia-action is the great and uncontrollable activity of the muscular system; that of curare, of iodide and sulphate of methyl-strychnium, and, as we shall presently see, of several other similarly modified poisons, is the flaccid and motionless condition caused by the impossibility of exciting muscular action through the nervous system. So opposite

are their effects that physiologists look upon curare as a powerful counter-agent to strychnia, while physicians have employed it with success in the treatment of strychnia-poisoning and of tetanus. It is remarkable that by so simple a chemical process so thorough a change should be produced in physiological action."

The same line of experiment was adopted in the case of brucia, thebaia, codeia, and morphia, and with similar results. The convulsant action of morphia was destroyed by the addition of methyl iodide or sulphate to the group; and, strange to say, the alkaloid so modified did not produce hypnotic effects in man or other warm-blooded animals.

The results afforded by atropia, however, possess even greater interest than those which I have just referred to. Atropia, unlike the other alkaloids, not only acts as a convulsant, but also on the peripheral terminations of the motor nerves, though in a minor degree. The new compound produced when the alkaloid is treated with methyl iodide—the iodide of methyl-atropium—does not act as a distinct convulsant on the animal organism; but the action of the alkaloid upon the motor nerves is greatly exalted by the addition, without interfering with the mydriatic action of atropia.

The general conclusions arrived at in the course of this highly important investigation were—

1. That the addition of methyl iodide or sulphate to the alkaloid molecule profoundly alters the physiological action of the body.

2. That the methyl derivatives of all the alkaloids act on the peripheral terminations of the motor nerves.

The first of these results is evidently of chief importance for our present purpose, as my aim in this portion of the lecture has been to show that the result of chemical addition to unsaturated molecules is to materially modify their physiological action.

We know so little of the chemical constitution of these alkaloids that we are unable to assert that the addition of a methyl compound fully saturates the molecule. But even if, strictly speaking, *saturated* by methyl iodide, the attractive powers of the components of the bodies are not necessarily fully *satisfied*—a distinction the value of which I endeavoured to point out in referring to the experiments with the sodic phosphates. The convulsant action of any of the alkaloids experimented with is unquestionably connected with the existence of two unsaturated "bonds" which tend strongly to attach the molecule to other matter. Save in the case of atropia, this power of attachment is so great that the

widely different paralysing action scarcely comes into play until the molecule has been saturated with C H₃ I. In atropia the paralysing action rises more nearly to the level of the convulsant, so that in that alkaloid we have, as it were, a polar condition.

The train of thought suggested by these considerations is highly attractive, but were we to pursue it we should probably be led beyond the bounds of legitimate scientific speculation. I must, therefore, rest content with having suggested a probable cause for the residual and distinct action of the alkaloidal groups after the saturation by methyl iodide or sulphate.

CONCLUSION.

The evidence adduced in support of the four propositions which I sought to establish in these lectures could have been largely supplemented, but I preferred to select some well-marked cases for comment rather than to attempt to deal with the whole of this wide subject in the necessarily short time allotted to these discourses. I trust, however, that the observations made have sufficiently shown that the study of the chemical constitution of medicinal substances, and of bodies not yet used in therapeutics, should go hand-in-hand with the study of their physiological action, as the latter is evidently dependent to a great extent on the mode of grouping of the components of the molecules.

Having thus connected chemical constitution, rather than mere composition, with physiological activity, it is reasonable to expect that the effect produced by an active body within the organism can be explained on ordinary chemical principles. In dealing with several of the cases of physiologically active bodies I have shown that we can trace out to a certain small extent the probable *rôle* performed by some of the compounds within the organism; but our knowledge of the chemical changes involved in vital processes is as yet too limited to justify us in going far in this direction. I may, however, supplement any observations made under special heads by pointing out here that if we exclude from consideration those bodies which exert a local corrosive action on the gastro-intestinal surface, and those which seem (like carbon dioxide) to act *chiefly* by quenching oxidation, the remaining substances which are chemically and physiologically active can directly or indirectly interfere with the production of nerve force in one or more of at least five different ways:—

1. By directly removing oxygen from the blood, and thus interfering with the ordinary processes of oxidation.

2. By uniting with hæmoglobin to form compounds analogous to carbonic-oxide-hæmglobin, or to Ray Lankester's cyano-hæmoglobin, thus preventing the normal production of oxy-hæmoglobin.

3. By combining with effete material and increasing the difficulty of its removal by oxidation or otherwise.

4. By withdrawing from living tissue material essential to its vitality.

5. By chemically combining with living tissue and interfering with the performance of its proper function.

In concluding this section, and with it these lectures, permit me to express the hope that the outline I have endeavoured to give of some recent work on the border-land between chemistry and physiology, may serve to show that the progress of true therapeutics must, in the future, largely depend on the extension of our knowledge of the chemical changes involved in the so-called vital process.

ART. XXI.—*On the Use and Abuse of the Nasal Douche, and on the alleged danger attending the Introduction of Fluids into the Nasal Passages.* By JAMES PATTERSON CASSELLS, M.D., M.R.C.S., London; Fellow of the Faculty of Physicians and Surgeons; Surgeon to, and Lecturer on, Aural Surgery at the Dispensary for Diseases of the Ear, Glasgow.

THE object of this communication is to consider the question of the use and abuse of the nasal douche, and to inquire if, as is alleged, there be danger attending the introductien of fluids into the nasal passages, and, further, to examine the evidence upon which this allegation is founded.

I include under the well-known name of nasal douche not only the syphon douche of Weber—*the* douche *par-excellence*—but all the usual modes of introducing fluids into these passages—viz., the anterior douche of Gruber, the posterior one of Pomeroy, and the so-called " snuffing-up " process, all of which are, indeed, so many modifications of the original douche of Weber.

It may be assumed, I think, on behalf of the Weber's nasal douche, as introduced to the notice of the profession by Dr. Thudichum,* that it has been, and still is, held in high repute,

* See Lancet, 24th Nov., 1864.

more especially by those who have used it frequently, and found it to be, what it really is, a highly effective and not unpleasant mode of treating certain affections of the naso-pharynx—so satisfactory, indeed, as to make it difficult, if not impossible, for any one to devise an equally efficient substitute, were such a step deemed necessary. This being so, it was somewhat startling to be told by an authority of no mean eminence, and whose opinion, therefore, commends itself to our highest respect and consideration, that there was a danger of doing serious injury to the ears in using the Weber's douche, and that, therefore, " its *use* (the italics are mine) ought to be discountenanced by the profession." When, therefore, Professor Roosa, of New York, the authority above referred to, had thus shaken our confidence in the assumed safety of this proceeding, by appearing to prove that its use, " even with proper precautions," was productive of serious ear disease, many of the profession who had used the douche, hitherto without fear, abandoned this manœuvre altogether, and found peace of mind in the assumed safety of the use of such modifications of this douche as have been already named. Others allowed the warning thus given to pass unheeded, while others, again, some of them men of eminent authority, questioned the justness of Roosa's opinion just stated, on the grounds that the evidence which he adduced in support of this opinion was insufficient and incomplete, and that it did not warrant such a conclusion.[b]

Nevertheless, enough had been said, whether rightly or wrongly, by Roosa, on this subject, to shake the confidence of all but a very few in this mode of douching the nasal passages and naso-pharynx; and, as already mentioned, many found a refuge in the use of one or other of its modifications. But even these modifications are assailed in quite recent times by one so eminent as Dr. Buck of New York, who, besides, agreeing in the opinion of Roosa, relative to the *use* of the douche of Weber causing serious ear disease, also raises the question of the safety of introducing fluids *at all* into the nasal passages.[c] This latter-named writer recommends, in the article referred to,[d] as the result of his experience, that *all* the modes of introducing fluids into the nasal passages, at present in use in

[a] See Arch. für Aug., u. Ohrenheil. Bd. I., 1867. Also his recently published Treatise on the Ear, p. 291.

[b] See critique of Roosa's position in Arch. f. Ohrenheil. Bd. I., N.S. Also Prof. Elsberg's reply to Roosa in Arch. f. Aug., u. Ohrenheil. B II., p. 77.

[c] See New York Medical Record, 24th March, 1877.

[d] On the Danger Attending the Introduction of Fluids into the Nasal Passages.

the treatment of their diseases, ought to be abandoned, because of the danger that may accrue to the ears therefrom; even the simple, efficient, and seemingly innocent "snuffing-up" process, so much in use, is condemned with the others; and the "swabbing-out" process is recommended in their stead; a method, by the way, less efficient for treating naso-pharyngeal affections than the douche, while *it* is not altogether free from danger. In these circumstances, when a therapeutic proceeding of acknowledged value is threatened with extinction, it seems high time, in the interest of general and special practitioners as well, to inquire if there be good grounds for this new alarm, by examining the evidence upon which it is based.

Concerning the opinion of Roosa, that "the use of the Weber's nasal douche ought to be discountenanced by the profession," . . . "even with proper precautions" taken as to the mode of employing it, I have to say, with every respect for the deservedly great authority of this writer, that I do not share in his apprehensions regarding its evil effects on the ears, for the best of all reasons— viz., that in my hands it has invariably proved itself to be a perfectly safe, and not unpleasant proceeding. In these circumstances I would suggest, if the evidence which he offers in support of the position that he holds, with regard to this measure, be not in proof of the *misuse* rather than the *use* of this douche. To *use* a right remedy wrongly in a right case, or rightly in a wrong one, and to expect good results to follow, is surely a little unreasonable; but, in such circumstances, to blame the *use* of the remedy, seems to me to be as unjust as it is unscientific; the *misuse* of a thing cannot, I submit, be urged as a hindrance to its *use*.

In speaking thus, I assume that all the evidence that Professor Roosa has to offer in support of his denunciation of the *use* of the nasal douche, is similar in character to that which he publishes in his recent work on the ear; if this be so, then I have to ask, judged by the canons of a scientific method of investigation, is there in the history of the case, reported on page 292 of his book, the smallest evidence that the sufferings of the patient, as there detailed, were, even in the most remote way, the result of the *use* of the nasal douche? As for the "analysis of reported cases," tabulated on page 296 of the same work, I have to say, that its incompleteness deprives it of all value in the determination of the question before us. To condemn, therefore, the *use* of a recognised and valuable therapeutic measure, as Professor Roosa has done, on evidence so weak and so incomplete, is surely unwarrantable.

Let us now turn our attention to the consideration of the position taken up by Dr. Buck, who, as has been already stated, alleges that there is "danger attending the introduction of fluids into the nasal passages" to the ears, and to the nature of the evidence which he adduces in support of that position. As in the case of Professor Roosa, so it is in that of Dr. Buck, the evidence does not warrant the conclusion. It is too weak and too inconclusive to be employed in a condemnation so wholesale as that indulged in by this latter-named writer; and if it proves anything at all, it proves, even more conclusively than Roosa's evidence, that, in all the cases reported by Buck, the fluids were passed into the nasal passages in improper cases, or used improperly in cases that justified their use; here again showing that the terms use and misuse have been misplaced as well as misapplied.

To show that I am not speaking rashly, nor without warrant, I shall place before the reader a brief analysis of the evidence, as follows:—Ten cases of naso-pharyngeal catarrh are reported, in which it is alleged that the ear disease, from which each patient suffered, was the direct result of the nasal douche in one or other of the modifications that have, at the outset of this paper, been named. It is, however, to be noted that in all these cases no notice has been taken of the state of the ears *before* the nasal douche was employed; such a gap in the evidence is suggestive of many questions relative to the state of these organs prior to the use of the douche. In four cases in which the douche seemed admissible, the patient "blew his nose" *immediately* after passing the fluid through the nasal passages; one used cold water alone through the nostrils, the water being of "the temperature of the croton in April;" one used the posterior nasal-douche in "an active naso-pharyngeal catarrh," and so on; the remaining four cases are so briefly reported as not to admit of analysis. Here we have six out of ten cases in which the douche was clearly and unmistakably misused, in which bad results to the ears could have been foretold! So much then for the evidence upon which the *use* of the nasal douche is condemned! As the experience of each man is a fair measure of the worth of his opinion on any given point in which he has had experience, and ought to be a law unto himself, if it be not one to others as well, I shall now briefly show that I have not spoken without some little experience of the use of the nasal douche.

In my out-door *clinique* here, I have treated, by *my own hands*, upwards of 2,300 cases of ear disease, in which the nasal passages

were more or less affected with one or other of the many forms of catarrh. These all occurred in the persons of poor and mostly ill-fed, badly-clothed people of all ages. Two-thirds of that number have used the nasal douche in one or other of its forms; the syphon douche, when necessary, was always used upon the patient by myself; so with the anterior and posterior modifications of it:— the " snuffing-up" process always was used by the patients for carrying out home treatment; most of the patients used the douche for several weeks each on an average, some, indeed, have used it without intermission for two years. The report that I have to record is, that I have neither seen nor heard of an untoward result, not even a single complaint. In my private practice I have the like experience and the same result to report. This being so, it is, perhaps, quite warrantable to ask that I should account for this success in the use of an operation that seems in other hands to have proved hurtful. To this I reply, that I never use it except in appropriate cases; never to trust the use of the syphon-douche to the patient, but in every case to do the operation upon the indivi-dual myself; self-use in this as in many similar circumstances mostly means self-abuse. After deciding that the case is one in which the douche is admissible, I observe the following precautions, which, I may add, are applicable to the various modifications of the process. To have the fluid to be used non-irritating—of a density greater than the serum of the blood—about 90° Fh. in temperature, and *never to use pure water alone.* To give the column of fluid a fall from a point about one foot above the level of the patient's nose, patient meanwhile leaning forward and breathing short rapid breaths, about 40 per minute, interrupting the flow of the fluid every few seconds to allow of the patient resting, and to permit of the nostrils being sniffed out from behind by a succession of violent expirations through the nostrils, *the mouth being closed,* stopping the fluid from passing into the nostrils the instant that the patient ceases to breathe, as I have directed, or on any involuntary act of swallowing taking place on the part of the patient—finally, in all cases, never to allow the patient to blow the nose after using the douche till all the residual fluid has been expelled from the nasal passages by oft-repeated and strong expirations through them with the mouth closed. Lastly, never to begin the douche till the patient thoroughly comprehends the part that he or she is expected to perform in the course of the proceeding.

To sum up what has been said, I may repeat that the evidence

adduced by Drs. Roosa and Buck does not warrant the condemnation of the *use* of the nasal douche, nor does it show that there is danger attending the introduction of fluids into the nasal passages; on the contrary, it shows that the misuse of a recognised and valuable therapeutic measure has been followed by bad consequences, which is no more than has happened and must happen in like circumstances, as the history of Eustachian catherism shows, for example; yet who, I may be permitted to say, would feel warranted in asking that the *use* of the catheter should be discountenanced because dangerous consequences has resulted from its misuse! My own position with regard to the nasal-douche in all its modifications is quite clearly defined—with proper precautions, both on the part of the operator and on that of the patient, no harm need result; nay, its use under such circumstances is as free from danger as is the use of the Eustachian catheter, when prudence and skill guide the judgment and the hand of the operator.

THE THERAPEUTICAL PROPERTIES OF GLYCERINE.

M. CHATILLON has just published his researches on glycerine as a food. In small doses it increases the weight as it lessens waste of tissue, in consequence of its being oxidized in the lungs in preference to the fat of the body. Even the nitrogenous substances are more slowly consumed, as is shown by the diminished quantity of urea excreted in the twenty-four hours. Glycerine is a stimulant to the digestive functions, well tolerated, quickly digested, and absorbed so completely that unless taken in large quantities hardly any is found in the blood or urine. Elimination by the kidney begins within an hour of the time it is taken. It produces neither glycosuria nor albuminuria, and it has a laxative tendency. In large doses, or if taken suddenly into the stomach, it causes symptoms somewhat like those of acute alcoholism, but if taken gradually it only raises the temperature a little. The proper dose ranges from half an ounce to an ounce a day, and, in many cases, M. Chatillon would recommend it as a substitute for cod-liver oil. It possesses the pharmaceutical property (as before noticed in this Periscope, p. 304) of preventing iron from combining chemically with cinchona, so that the three can be given in a mixture.—*Le Progrès Médical.*

S. W.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

The Functions of the Brain. By DAVID FERRIER, M.D., F.R.S.
    London: Smith, Elder, & Co. 1876. 8vo, pp. 323.

THIS book, which contains Dr. Ferrier's most recent conclusions
on the functions of the brain, cannot fail to excite the greatest
interest in the mind of every physiologist. Although to Hitzig
and Fritsch belongs the priority of the discovery of the electric
excitability of the brain, and of definite localisation of function in the
different parts of the cortex of the hemispheres, yet this detracts but
little from Dr. Ferrier's merit; for he has so far improved the
methods of experiment, he has so much more thoroughly explored
the entire intra-cranial mass, he has operated on such a number
and variety of animals, and his results are so much more numerous
and definite than those of the German experimenters, that he
cannot be considered as owing much to the researches of his prede-
cessors. While other investigators have operated on animals
belonging to only one or a few species, Dr. Ferrier has extended
his experiments to all classes of vertebrates, and he alone has had
the inestimable advantage of operating on a very large number of
monkeys. He has thus been able to reconcile the discrepant
results obtained by many of those who repeated his earlier experi-
ments, and has been enabled to form large generalisations on the
gradual evolution of the central nervous system, and on the pro-
gressively increasing inter-dependence of its parts as we ascend the
animal scale, which it would have been impossible to arrive at from
a less extended research.

In his preface Dr. Ferrier says:—"My chief object in this book
has been to present to the student of physiology or psychology a
systematic exposition of the bearing of my own experiments on
the functions of the brain. To do this satisfactorily I have
thought it necessary to consider the functions of the cerebro-spinal
system in general, with the view more especially of pointing out
the mutual relations between the higher and the lower nerve-
centres." Accordingly, in the earlier chapters we find a sketch of

the structure of the brain and spinal cord, and a short, but suf-
ficiently complete, account of the functions of the spinal cord and
medulla oblongata.

The corpora quadrigemina, pons Varolii, and cerebellum are first
considered together, and the phenomena are described which follow
ablation of the hemispheres in the different classes of animals. In
frogs, fishes, and birds a complete loss of spontaneous action is
observed, but these animals preserve their equilibrium, and, if stimu-
lated, perform perfectly co-ordinated movements, and jump, swim,
or fly as well as if they still possessed their hemispheres, avoiding
obstacles in their path. In rabbits and guinea-pigs the phenomena
are somewhat different. When recovery has taken place from the
shock of the operation, it is found that the muscular power of the
limbs, particularly the fore-limbs, is greatly impaired, but the animals
can maintain themselves on their legs, and can regain their equili-
brium if it is disturbed. If pinched they bound forward, but have
no power to avoid obstacles. The pupil reacts to light, a loud sound
causes twitching of the ear and starting, a bitter substance intro-
duced into the mouth causes movements of disgust, and ammonia
applied to the nostrils causes retraction of the head. When one of
these animals is pinched it responds not only by movement but by
cries, which are repeated and prolonged, and have a plaintive
character, and thus differ from the cries uttered by animals in
whom all the parts above the medulla oblongata have been
destroyed. But in rodents, as in lower animals, all power of per-
forming spontaneous movement is lost after removal of the hemi-
spheres. We thus see that in rodents, frogs, fishes, and birds, " the
power of maintaining the equilibrium is retained; co-ordinated
locomotive actions and emotional manifestations are capable of being
excited by impressions on sensory nerves, essentially, if not altogether
to the same extent, in all." In cats, dogs, and higher animals the
prostration and loss of muscular power is so great that any inde-
pendent activity of the lower centres in maintaining equilibrium or
progression practically ceases. There is, however, still emotional
response to sensory impression, and this makes it probable that the
other forms of functional activity are not absent but merely sus-
pended.

The loss of spontaneity after ablation of the hemispheres is
admitted on all hands; but there is some difference of opinion as
to the nature of the emotional and other responsive phenomena
called forth by stimulation, many holding that these indicate the

maintenance of a certain degree of conscious sensation, while others think that they are mere reflex manifestations. Dr. Ferrier admits that this question cannot be as yet decided by experiments on animals, but thinks that the experiments of disease in man settles the question against the persistence of conscious sensation after the removal of the hemispheres. For when by lesion of the crus cerebri or posterior part of the peduncular expansion, one of the hemispheres is practically detached from its mesencephalic connexions, although thought and speech remain intact, " the individual has absolutely no consciousness of tactile impressions made on the opposite side of his body, *however much he may strain his attention to receive them.*" It is proposed to discard the term sensation altogether, to use the term *æsthesis*, to signify a mere physical impression on the centres of special sense, and the term *noesis* to signify a conscious impression. " The reaction of the mesencephalic and cerebellar centres might be termed *æsthetiko-kinetic*, and be thus distinguished from the *kentro-kinetic* or excito-motor action of the spinal cord on the one hand, and the *noetiko-kinetic* action of the cerebral hemispheres on the other."

The functional manifestations of the mesencephalic centres are three:—1. The maintenance of equilibrium; 2. Co-ordination of locomotion; and 3. Emotional expression. Each of these is treated of at length. The maintenance of equilibrium is shown to involve the operation of three factors:—1. A system of afferent nerves; 2. A co-ordinating centre; 3. Efferent tracts in connexion with the necessary muscular apparatus. Lesion of any or all of these destroys the faculty of equilibration. The afferent apparatus is of a compound nature, but consists mainly of three or, perhaps, four great systems:—1. Organs of reception and transmission of tactile impressions; 2. Organs for the reception and transmission of visual impressions; 3. The semicircular canals of the internal ear, and their afferent nerves; and, 4. Probably, the organs for the reception and transmission of visceral impressions. The discussion of the disturbances of equilibrium which follow lesions of these systems forms, when taken with the account of the vertigo which follows irritation or injury of the cerebellum, one of the most interesting parts of the book.

After studying the mesencephale as a whole, the functions of the different systems of which it is composed are made the subject of consideration. The corpora quadrigemina are anatomically closely related to the optic tracts, and it has been long known that destruc-

tion of the corpora quadrigemina on one side destroys vision in the
opposite eye, but the blindness in this case is due merely to inter-
ruption of conductors, for the true perceptive centre for vision
is situated in the cortex of the hemispheres. After destruction of
the corpora quadrigemina the pupils are widely dilated, and no longer
respond to light; hence it is probable that the reflex centre for the
movements of the iris is seated in this part of the brain. Besides
these ocular effects, injury of the corpora quadrigemina causes in
all classes of animals marked disturbances of equilibrium and of
locomotion. The effects of electrical irritation of the corpora
quadrigemina are very remarkable, and vary according to the posi-
tion of the electrodes. In monkeys "irritation of the nates on one
side causes the opposite pupil to become widely dilated, followed
almost immediately by dilatation of the pupil on the same side.
The eyes are widely opened and the eye-brows elevated. The eye-
balls are directed upwards and to the opposite side. The head is
moved in the direction of the eyes. The ears are strongly retracted.
With continuance of the irritation the tail becomes elevated, the
legs extended, and the jaws firmly clenched, with the angles of the
mouth retracted to their utmost. The arms are approximated to
the sides, and drawn back flexed at the elbows. Ultimately, when
the stimulation is kept up, a state of complete opisthotonus is
produced. Irritation of the testes or posterior tubercles produces
the same effects—but, in addition, cries are excited, varying in
character from a short bark, caused by the slightest contact of the
electrodes, to all varieties of vocalisation, when the stimulation is
continued. The motor effects are shown—first, on the opposite
side of the body, but ultimately both sides become affected by the
unilateral irritation." In other animals the results of irritation are
similar. In the explanation of the phenomena which follow injury
or irritation of the corpora quadrigemina, it is argued that these
bodies are centres for the reflex expression of feeling or emotion.
Most of the results of irritation are the same as those which follow
irritation of sensitive nerves, and it has been shown that the effects
produced on circulation and respiration are similar in both cases.

Much doubt has lately been thrown on the celebrated experiment
of Flourens, by which he supposed that he had demonstrated the
function of the cerebellum to be the co-ordination of the movements
willed by certain parts of the nervous system and executed by
others. Ferrier, after a careful examination of all the arguments
for and against, and as the result of his own numerous experiments,

finds " that with a remarkable degree of uniformity in all classes of animals, mechanical lesions of the cerebellum induce, in the first instance, if not permanently, such disorders of equilibrium that station and locomotion become difficult and sometimes altogether impossible. It is not that the muscles are paralysed, or that co-ordinated movements of locomotion are rendered impossible, for the animal still retains the power of voluntary motion, and the co-ordinated combinations of muscular contractions concerned in locomotion may be willed or excited as before, but these are no longer co-ordinated with the position of the body in space, and hence the animal, in its attempts at locomotion, reels, staggers, or flounders about in the most helpless manner" (p. 91). These results have been explained by some as the result of destruction—by others as that of irritation—of the cerebellum or subjacent parts; but Ferrier shows that both irritative and destructive lesions are capable of causing disturbance of equilibrium, but that the effects are opposed to each other.

When the cerebellum is divided antero-posteriorly in the middle line, or when symmetrical lesions are made on both sides, the disturbances of equilibrium are slight; but when the lesions are unsymmetrical, the equilibrium is seriously impaired, and overthrown in one direction or the other according to the seat of lesion. Injury of the anterior part of the middle lobe causes the animal to fall forwards, while injury of the posterior part causes retraction of the head and tendency to fall backwards. There is some discrepancy among observers as to the result of unilateral injury of the cerebellum, but the weight of evidence would seem to show that if the lesion is severe the animal rotates on its long axis towards the injured side; while, if it is slight, there is a tendency to fall on the opposite side. Electrical irritation in monkeys and other animals causes deviations of the head and eyes. When the anterior part of the middle lobe is irritated, the eyes move directly upwards, the head is thrown backwards, and a tendency to extension of the legs occurs. If the electrodes be placed on the posterior part of the middle lobe, the eyes move directly downwards, and the head is inclined forwards. Irritation of one lateral lobe causes the eyes to move to the side of irritation, and produces movements of the limbs on the same side. When the flock lobe is irritated the eyes rotate on their antero-posterior axes. These phenomena explain the nystagmus or permanent deviation of the eyes so commonly met with in man, in cases of disease or injury of the cerebellum.

Many years ago, Purkinje described peculiar vertiginous sensations which are produced in man when a galvanic current is passed transversely through the skull in the cerebellar region. These have been recently very fully investigated by Hitzig. When the current is passed from right to left, at the moment of its closure the body and head sink towards the right (anode) and the eyes rotate in the same direction, while objects appear to whirl across the field of vision towards the left, or in the direction of the current. If, however, the eyes be closed, although the body may really be inclined to the right, yet there is a feeling as if it were whirled from right to left, or as if the basis of support on the left side were withdrawn. If the current be broken or reversed, the direction of all the movements is altered to the exact opposite of what they were before. These phenomena are probably due to irritation of the cerebellum at the side on which the anode is placed when the current is closed, and at the side next the kathode when it is broken. Our space will not admit of our following Dr. Ferrier through his explanation of these phenomena, but we can barely state his conclusion. He believes that the cerebellum is a compound arrangement of centres which together regulate the muscular adjustments which maintain the equilibrium of the body. Thus, in the anterior part of the middle lobe is the centre which resists the displacement of the body forwards. When this centre is irritated, the eyes are rotated upwards and the head thrown backwards, just the movements which would be performed for the restoration of equilibrium if the body tended to fall forwards; while if the centre in question suffers a destructive lesion, the body falls forwards, from the unbalanced action of the centre, which resists backward rotation, and which is seated in the posterior part of the middle lobe. Similarly in each lateral lobe is a centre which resists a falling away towards the same side. The action of each centre is automatic, or is an example of what Ferrier calls æsthetiko-kinesis. The special stimulus which calls each centre into action is a tendency to displacement of the equilibrium round a horizontal, vertical, or intermediate axis. The muscles called into play are the ordinary muscles of the body. These are not paralysed when the cerebellum is destroyed, but they no longer act automatically in maintaining the equilibrium of the body; they can, however, be made to perform this function under the influence of the will. This explains why, to a large extent, the power of co-ordinated motion returns after

injury of the cerebellum, and why, in slowly progressive disease of this organ disturbance of equilibrium may be slight or altogether absent. It also explains the readiness with which fatigue is felt in these cases—a phenomenon which has led some persons to look on the cerebellum as an organ whose function is to strengthen the other nerve-centres in their action. The view which attributes to the cerebellum a special influence on the generative function receives no support from Dr. Ferrier.

The view of the functional equivalence of all parts of the cerebral cortex put forward by Flourens was first made doubtful by the discovery that loss of the faculty of speech coincides with lesion of a certain definite part of the left side of the brain. It was, however, Fritsch and Hitzig who first showed experimentally that by irritation of limited portions of the surface of the hemisphere constant and definite muscular movements could be produced. Dr. Ferrier, repeating these experiments, using the faradic instead of the continuous current, has discovered not only a great number of new motor centres, but has also localised the various kinds of sensation in definite parts of the brain, and has in every direction vastly extended our knowledge of cerebral physiology. It has been objected by many that the results obtained by electric irritation of the cortex (mechanical irritation being without effect) were really due to stimulation of the basal ganglia, by currents conducted through the thickness of the hemispheres. This is disproved by various considerations, but chiefly by the fact that those parts of the cortex which lie nearest to the corpus striatum are altogether unexcitable even by strong currents, while the feeblest irritation of the upper and outer surface of the hemispheres calls forth movements of the opposite side of the body.

After a preliminary chapter, chiefly on the methods of experimentation, and the objections which have been raised to these, the author describes the phenomena witnessed when electric irritation is applied to the cerebral hemispheres and basal ganglia in monkeys, dogs, jackals, cats, rabbits, guinea-pigs, rats, pigeons, frogs, and fishes. The experiments are given briefly, the reader being referred for further details to the author's papers presented to the Royal Society. In the next chapter the hemispheres are considered physiologically, and the results of destruction of the cortical centres are described, and compared with those obtained by their stimulation.

Visual Centre.—When the angular gyrus is stimulated, move-

ments of the eye-balls are noticed, with movement of the head to
the opposite side, and, frequently, contraction of the pupils. These
movements are reflex, and due probably to the excitation of sub-
jective visual sensation, for destruction of this part of the brain
causes no paralysis, but blindness in the eye of the opposite side.
This blindness is not permanent, the corresponding centre on the
other side compensating, probably through the media of the lower
ganglia, for the loss of that which has been destroyed. If the
angular gyrus on both sides is destroyed, the blindness is complete
in both eyes, and permanent. The results of experiment, as well
as the observations of disease, tend to confirm the ingenious view of
Charcot as to the course of the fibres of the optic nerves. Accord-
ing to this the fibres from the outer half of each retina pass into
the optic tract of the same side, and decussate in the corpora
quadrigemina; those from the inner halves decussate in the chiasma;
all the fibres from each eye finally reaching the angular gyrus of
the opposite hemisphere.

Auditory Centre.—Irritation of the superior temporo-sphenoidal
convolution causes sudden retraction or pricking up of the opposite
ear, wide opening of the eyes, dilatation of the pupils, and turning
of the head and eyes to the opposite side. These movements, like
those caused by irritation of the visual centre, are reflex, and due
to subjective auditory impressions, resembling those caused when a
loud sound is produced near the ear opposite to the side of the
brain which is irritated. Destruction of the superior temporo-
sphenoidal gyrus causes no paralysis, but deafness in the opposite
ear.

Tactile Centre.—It is impossible experimentally to separate the
hippocampus major and uncinate convolution. Owing to the deep
position of these parts it is difficult to reach them for purposes of
electric irritation, and the results of this method of experimentation
are not very precise. Monkeys showed signs of restlessness and
uneasiness, and turned the head to the opposite side and backwards,
as if conscious of some uncomfortable tactile sensation, chiefly in the
opposite limbs. Similar observations were made on other animals.
The results of destructive lesions, however, are more definite.
When the hippocampal region is destroyed there is complete hemi-
anæsthesia of the opposite side. There is no loss of sight or
hearing, no paralysis, but nevertheless the limbs become practically
useless, since the tactile sensation by which their movements are
guided is lost. The anæsthesia resulting from destruction of the

hippocampus seems to be permanent. The localisation of tactile sensation in the cortex, and not in the basal ganglia, is confirmed by the fact that division, experimentally or by disease, of the posterior part of the internal capsule, or the radiating white fibres which lie external to the optic thalamus, causes anæsthesia of the opposite side. Here the effect is due, not to destruction of centres, but to division of conductors, and the loss of common or tactile sensation is accompanied by that of special sensation of every kind in the side opposite to the lesion. These results are similar to those observed in hysterical hemi-anæsthesia and that from alcoholism.

Centres of Smell and Taste.—The limits of these two centres respectively cannot be accurately localised, but they are both seated in the tip of the temporo-sphenoidal lobe or subiculum cornu Ammonis. Irritation of this part causes a torsion of the lip and partial closure of the nostril on the *same* side. This is a reflex movement indicative of intense subjective olfactory sensation, and similar to that produced by direct application to the nostril of a powerful or disagreeable odour. Destruction of the lower end ot the temporo-sphenoidal lobe on one side caused loss of smell in the nostril of the same side. Taste could not be investigated in only unilateral lesion, but when the centres on both sides were destroyed, taste was completely lost, as well as tactile sensibility of the tongue. Smell also was lost on both sides, while acetic acid, introduced into the nostrils, caused motor reaction and lachrymation from irritation of the fifth nerve. The loss of smell on the same side as the lesion is in accordance with the anatomical connexions of the roots of the olfactory bulb with the corresponding hemisphere, without decussation. The olfactory chiasma, described by Meynert as existing in the anterior commissure, contains fibres passing from the bulb of one side to that of the other, and from the centre of one side to that of the other, but not fibres from the bulb of one side to the opposite centre. If this were so, section of the anterior commissure should abolish smell on both sides—an effect which should also result from unilateral destruction of the subiculum. But this is not the case. Several instances are on record of aphasia, right-sided hemiplegia, and unilateral anosmia, always on the side of lesion. But in cerebral hemi-anæsthesia, as already noticed, together with tactile sensation, smell is lost in the side *opposite* to the lesion. Ferrier explains this by Magendie's observation that, after division of the fifth nerve, smell is lost—the integrity

of the nerve of common sensation being essential for the performance of the function of that of special sense. In one of Magnan's cases of hemi-anæsthesia, smell diminished *pari passu* with the loss of tactile sensibility in the nostril, and both disappeared together. The interesting observation is made that a reflex influence on the lachrymal gland can be exerted through either the fifth nerve or the olfactory; for in Magnan's case, when common sensation and smell were lost, acetic acid caused lachrymation, evidently through the olfactory nerve, although this nerve was incapable of conveying impressions of smell, while, in one of Ferrier's monkeys, in which on one side the centres both of touch and smell were destroyed, and on the other only the olfactory centre—acetic acid in one nostril gave no reaction, in the other profuse lachrymation.

Irritation of the occipital lobes is without effect, and their removal causes loss of neither sensation nor motion. But after this operation the animal refuses food, while the desire for drink still continues. The proximate cause of thirst is supposed to be a dry condition of the fauces, and consequently belongs to the tactile sensations. These not being interfered with by the operation, the sense of thirst is not lost. But the proximate cause of hunger is a local condition of the stomach. This belongs to the class of visceral or organic sensations which are not abolished in cerebral hemi-anæsthesia. It is probable, not only from the experiment quoted, but from other reasons, that the centre for these systemic sensations is situated in the occipital lobes.

Motor Centres.—On irritation of certain spots of the brain which lie in monkeys, for the most part in the convolutions which bound the fissure of Rolando, certain constant and definite movements of the limbs, face, head, tongue, &c., result. Similar centres are found in the brains of other animals, and give rise when irritated to completely homologous movements. Merely from these movements it does not follow that the irritated spots are motor, for the muscular contractions might be caused in a reflex or indirect way. But " the definite purposive character clearly perceivable in many of the movements, and their correspondence with the ordinary volitional activities and peculiarities of the animals, apart from other considerations, point rather to the conclusion that they are the result of the artificial excitation of the functional activity of centres immediately concerned in effecting volitional movements, and as such truly motor." This is proved by the fact that destruction of these centres paralyses just those movements which are called forth

by their irritation. Such paralysis is stated by Ferrier to be permanent in monkeys, although it is not mentioned how long the animals have survived the operations, except in a few instances, in which the period was very short. The facts of disease in man would tend to show that in him, too, destruction of the cortical motor centres induces a permanent paralysis. In dogs the paralysis is at first nearly complete, but is soon recovered from, while in rabbits the effects are still slighter and more transitory. Different explanations of the recovery from the paralysis have been given. By some, the centres in the opposite hemispheres corresponding to those destroyed were supposed to take on the actions of the latter. But this is disproved by destroying the corresponding centres, first on one side, and then, when recovery has taken place, on the other. The last operation is found not to reproduce the former paralysis, but merely to cause loss of power on the side opposite. Again, it has been supposed that parts of the same hemispheres in the neighbourhood of the injury may take on the function of the portion destroyed. But this would involve the strange anomaly of a portion of the cortex being at one time a sensitive and at another a motor centre. There is no experimenter who has been so successful in preserving his animals after removal of portions of the brain as Professor Goltz. By repeated operations he has, in dogs, removed all the motor centres on both sides, and not only have the animals recovered so well as to be able to stand tolerably securely or even walk, as stated by Ferrier, but they run about, and perform all their ordinary movements in a surprisingly perfect manner. Goltz ascribes the primary paralysis not to destruction of motor centres in the cortex, but to an inhibitory action exerted by the irritation of the operation on deeper parts of the brain. Ferrier believes that those movements which are performed after removal of the cortical centres are only such as have become "automatically organised" in the lower centres, corpus striatum, mesocephale, and spinal cord. In the case of the lower animals almost all their movements are so organised; hence the slight and transitory effect of cortical lesions; in higher animals, on the other hand, almost all movements are largely influenced by volition; hence the permanent and complete paralysis observed in men and monkeys. We are aware that this bare statement does not, in any degree, do justice to Ferrier's views on this subject; but the length to which this notice has extended prevents our doing more than referring our readers to the work itself for the

full details of an exceedingly ingenious and well elaborated physio-logical argument.

In considering the views of Nothnagel and Hitzig, who attribute the loss of motility, after injuries of the hemispheres, not to true paralysis, but to a loss of muscular sense, Ferrier shows, we think very conclusively, that we have no consciousness of effort apart from the centripetal impressions due to muscular contraction, and that these are lost by destruction, not of motor, but of sensory centres.

The antero-frontal part of the brain is unexcitable by electricity. Its removal causes a peculiar deterioration of intelligence, shown by loss of the faculty of attention and of intelligent observation.

Irritation of the corpus striatum causes contraction of all the muscles of the opposite side of the body; localised movement cannot be produced by its irritation. Destruction causes complete hemiplegia of the opposite side—the effects, like those of the removal of the cortical centres, being more marked in higher animals than in those lower in the scale, whose movements are more automatic and are " organised" in still lower centres.

The optic thalamus is absolutely unexcitable. Its destruction causes anæsthesia of the opposite side. It is to sensation what the corpus striatum is to motion, the sensations and movements, which are differentiated in the cortical centres, being integrated in these basal ganglia. The varying effects which lesion of the optic thala-mus exerts on vision are probably dependent on the exact position of the injury in each case, and are, to a large extent, explicable by Charcot's theory as to the course of the optic fibres. " We should expect hemiopia of both eyes from a lesion occupying the region of the corpora geniculata; total unilateral blindness in the opposite eye if the lesion is central; and, if the lesion is very extensive, perhaps total loss of vision in the opposite eye, and nasal hemi-opia on the same side."

Our space will not allow us to refer to the chapter on " The Hemispheres considered Psychologically" further than to say that, like all parts of the book, it is replete with interest, not only to the physiologist but to the physician. In the concluding chapter the topography of the cerebral convolutions is described, and the pro-bable position of the cortical centres in the human brain pointed out, as well as the relations which the convolutions of the cerebrum bear to the different parts of the skull.

Du Darwinisme ou L'Homme-Singe. Par le Dr. CONSTANTIN
JAMES. Paris : E. Plon et Cie. Pp. 320.

THE plan adopted by the author of this work possesses at least the
merit of simplicity. Starting with an account of the creation,
derived, we are informed, from Moses and Ovid, Dr. Constantin
James treats the question with the air of a man who never felt any
difficulties on the subject. Having, therefore, little sympathy
with those weaker minds to whom these mysteries are not equally
clear, he regards any views different from his own as the wilful
attempts of infidelity to poison the springs of truth. As Mr.
Darwin has not paid sufficient deference to the writer of the Ars
Amoris, he is liberally charged with Materialism and Atheism. In
the commonplaces of denunciation Dr. James is quite at home,
but when he condescends to what he believes to be argument he is
less happy. He has no idea of what constitutes an induction. Mr.
Darwin had mentioned, as a collateral proof of the difference be-
tween races of men, the fact that the parasites which infest several
races are totally distinct, in disproof of which suggestion his critic
devotes an entire chapter, claiming to have refuted him because he
once noticed that leeches had fastened upon one twin sister which
had declined to bite the other! If the theory of evolution has
nothing to contend with beyond the limited information, wholesale
abuse, hackneyed ridicule, and self-satisfied dogmatism of the
" Collaborateur de Magendie," as he styles himself, its foundations
may be regarded as perfectly secure.

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*Extracts from the Records of the Boston Society for Medical
Improvement.* By the Secretaries of the Society, CHAS. D.
HOMANS, M.D., FRAS. B. GREENOUGH, M.D. Boston : 1876.

THIS is the sixth annual volume that the Society has issued, and,
as an indication of its activity and clinical work, it is very satis-
factory. Many of the cases are interesting, and most of them are
well reported, but there is little or no attempt at classification, and
the work possesses neither an index nor a table of contents. Dr.
Borland details an interesting case of Addison's disease in an un-
married girl, 19 years of age. She was admitted into hospital in
November, and died the following February. The discoloration of
skin was very marked : irritability of stomach and debility very great.
At the autopsy, after a careful search, the left supra-renal capsule
could not be found, and the right supra-renal capsule was atrophied.

PART III.

MEDICAL MISCELLANY.

Reports, Transactions, and Scientific Intelligence.

TRANSACTIONS OF THE MEDICAL SOCIETY OF THE COLLEGE OF PHYSICIANS.

SAMUEL GORDON, M.D., President.

GEORGE F. DUFFEY, M.D., Honorary Secretary.

Wednesday, 2nd May, 1877.

DR. AQUILLA SMITH in the Chair.

Elephantiasis Græcorum.

DR. HAWTREY BENSON exhibited a case of Elephantiasis Græcorum. The subject of the contagiousness or non-contagiousness of this disease was, he observed, a question of interest. Two works had lately been published on the subject, differing widely in the views they propounded. One of these, by Drs. Lewis and Cunningham, asserted that there was no evidence of the contagiousness of elephantiasis græcorum. The other, by Dr. Hansen, of Norway, maintained that it was contagious, and adduced the fact that since the segregation of persons afflicted with the disease in that country had been enforced, the number of cases had diminished considerably. He (Dr. Benson) thought that light was thrown on this question even by the single case which he was about to bring forward. In the year 1872 he exhibited before that Society a case of a man afflicted with this disease, who had been sent up to him by Dr. Stirling, of Thomastown.* He had been in India for twenty-two years, and came home affected with this leprosy. The disease did not show itself for a few months after he came home. He was under his care in the City of Dublin Hospital for about two months; and during that time he was exhibited to that Society, and a drawing, showing the nature of the disease, was taken. [Drawing exhibited.] He went home, and, after having been about a year and a-half at home, died of the disease. During that last period of his life his brother slept in the same bed with him, and wore his clothes. That brother had never been out of

* Dubl. Jour. Med. Sci. Vol. LIII., p. 290.

Ireland except once, forty-six years ago, when he spent some time in England. Leprosy had not appeared in the British Islands for some centuries, and therefore he could not have got the disease there. But he had got it, and he (Dr. Benson) had him there for exhibition that evening. No further proof was, in his opinion, wanting that he must have taken the disease from his brother. Five years ago, while on a pleasure excursion to Norway, he (Dr. Benson) stopped for some time in Bergen, where there was an hospital in which there were over 400 cases of leprosy, including both the anæsthetic and the tubercular forms. He saw a great number of the cases, and noticed the sepulchral tone of voice which they had—a peculiarity also observed in both the cases he was at present referring to, and which had also been observed by Dr. J. Little in cases that had come under his notice in India. The man whom he was now about to introduce had been for nine months in hospital in Kilkenny, under the care of Dr. Zachariah Johnson, after which he went under the care of Dr. Stirling. It would be seen that in his case the tubercles were more developed than those shown in Hebra's original plate. [The patient was then introduced.]

Dr. WALTER G. SMITH said that the statement that leprosy had been extinct for the last two hundred years in the British Islands should be greatly modified, for he believed that sporadic cases of it occurred. Three years ago, in London, he saw a case which he believed to have been a local development of true leprosy. In Dr. Hilton Fagge's catalogue of the models in the museum of Guy's Hospital it was stated that, although it was generally said that in Great Britain leprosy was extinct, yet occasional cases of it occurred in persons who had never been abroad. One was that of a man, a native of Cork, who had left Ireland twelve years before, and worked as a tailor in London and Croydon, and he showed the disease in a well-marked form. Another case was that of an Irishwoman, aged fifty-four, who had been living in England for thirty years. She had large brown patches on her body, much anæs-thesia, thick puffy lips, and swollen cheeks.

Dr. LYONS said he was familiar with the case of a gentleman in the south of Ireland who was attacked with this disease, and the circum-stances were such as to preclude the possibility of tracing it to contagion. They should be slow at arriving at a conclusion as to the contagiousness of the disease from a single case. His own impression was that it was not contagious. True leprosy was by no means extinct in Europe. It was prevalent amongst the population of Portugal, and caused the loss of fingers, toes, and even hands, and at the present day one of the institutions of Lisbon was a leper hospital. The term leprosy was somewhat vaguely used—in fact, it was difficult to say that any two

persons attached the same idea to it. The form which was charac-
terised by tubercular patches was, in its results, quite different from the
leprosy which he had seen in Lisbon and other places. He would con-
fine the term to what he believed to be the true disease, which destroyed
the parts from a constrictive process of the areolar tissue, and produced
loss of fingers, toes, nose, and so on. In Lisbon the experience of those
acquainted with the disease was that it was not in the slightest degree
contagious, and that it did not spread amongst families the individual
members of which were sporadically attacked.

Dr. BENSON, in reply, mentioned that the College of Physicians of
London lately appointed a committee to consider the question as to the
contagiousness of leprosy. They did so, at the instance of the Secretary
of State for the Colonies, who noticed, and was struck by the diversity
of opinion on the point between the book of Drs. Lewis and Cunningham
and that of Dr. Hanson. The committee arrived at the conclusion that
there was no evidence of the contagiousness, but they agreed with Dr.
Hanson in recommending the segregation of lepers on sanitary and
moral grounds. He (Dr. Benson) held that one fragment of positive
evidence on the point was worth a vast amount of negative evidence;
and, as far as he could ascertain, the evidence which the committee of
the College of Physicians had before them was altogether negative, and
the evidence which Dr. Lyons had before him was negative also.

Cruveilhier's Paralysis.

Dr. HAYDEN introduced for inspection by the members of the Society,
a patient suffering from Cruveilhier's paralysis. He was a young farmer,
about twenty years of age, of robust constitution, from the neighbourhood
of Athlone. Up to three years ago he had been remarkable for his
athletic powers in lifting weights, leaping, and running. He had been
repeatedly exposed to wet and cold, and frequently had to wade up to his
middle in water in driving cattle across streams, and so forth. Up to the
time just mentioned, he had been in good health; and then for the first
time he began to feel weakness in the arms, and found that he had not
the same muscular powers as before. He also had pains in the back. A
process of wasting of the muscles of the upper arms then set in, which
proceeded to the remarkable degree now visible. A peculiar feature in
the case was, that his forearms had not been at all affected. Correspond-
ing changes appeared in his lower limbs, the upper extremities of which—
namely, the thighs—were wasted, while the legs were not. There had
been, also, great wasting of the pectoral, the deltoid, and the other muscles
of the chest, which were quite flaccid. His powers of respiration had
been greatly reduced. His legs were plump, and his hands and forearms
had not undergone any atrophic changes; still, he had but an indifferent

power of grasping anything. His appetite was good; he could sleep well, and was not suffering any inconvenience, and had come to town merely for the purpose of having the disfigurements corrected. There was no wasting of the triceps muscle of the upper arm. There was no diminution of the electrical contractility of the muscles, but there was a remarkable diminution of temperature. The following measurement of his limbs had been taken that day :—Girth of right arm, 6¾ inches; of right forearm, 9½ inches; of left upper arm, 6½ inches; of left forearm, 9½ inches; of left thigh, 15½ inches; of left calf, 13 inches; of right thigh, 16 inches; and of right calf, 14 inches.

Dr. Foot remarked that Duchenne's Pathological Album, which contained a collection of photographs of nervous diseases, had a picture which, in every detail, was almost the ditto of the boy they had just seen. He had had a case under his care in which the disease commenced in the right upper arm, and the cause of it threw some light on the pathology of the disease. His patient was a young man, a sawyer by trade. A very heavy order for telegraph poles came into the establishment where he was employed, and the men were given extra wages to work extra time. This young fellow, who was not very strong, overworked himself sawing. He was obliged to walk a long distance to and from his work. The timber he had to cut was unusually hard; and his habits were not calculated to add to his strength, for he drank, smoked, and indulged his sexual propensities to a remarkable degree. Weakness, violent pains and wasting of his right arm set in, and soon affected the left, although he used the latter very little, if at all, in sawing, which showed exhaustion of the trophic nerves, rapid propagation of the degeneration across the cord, and the attacking of the trophic nerve-centres on the left side. The atrophy was confined to both upper arms, but was more on the right than the left. He had low temperature, and the other symptoms present in Dr. Hayden's case. It then began to affect the muscles of his lower limbs, as if the long walks had told on him. He then began to be late at his work, and, finally, had to give up work altogether. After six months' treatment, he got well enough to resume his work, but he could neither saw as much, or walk as fast as before. The treatment applied was—a constant current from a fifty-cell Smee's battery to the cervical muscles and the spine, the use of the hypophosphites, diligent rubbing and shampooing, cod-liver oil, and the greatest possible amount of nourishment. The muscles grew again, but never recovered their former tone; however, he was able to go back to his work.

Dr. Lyons alluded to a remarkable case of this kind which had been for a long time under observation in the Whitworth hospital. The age of the patient was somewhat more advanced than in the two cases they had

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just seen. The central division of the deltoid muscle had retained its volume in a much more distinct manner than he had seen in many cases that had come under his observation, and in the cases now shown. The way in which the muscles underwent atrophy was somewhat capricious. The same group of muscles was not always affected in the same way.

The CHAIRMAN asked Dr. Hayden was there any wasting of the abdominal muscles in his case.

Dr. HAYDEN said there was not. The sensibility of the wasted parts had not been affected either; on the contrary, the irritability of the affected muscles was rather in excess.

Remarks on the Proximate Causes of Sleep. By HENRY KENNEDY, M.B.; Physician to the Whitworth Hospital, Drumcondra; and to Simpson's Hospital.

IN the following paper I would make some remarks on the function of sleep, and particularly on its more immediate causes. That it is a function of primary importance need not be insisted on now, and it has this great peculiarity that divides it, if I may so say, from other functions—that during its continuance we are shut out from the external world, and a fourth, if not a third, of our lives is so occupied. It is a function, too, that, as medical men, we are constantly consulted about, and there is probably none which has made or marred more reputations than this one, for the number of diseases in which sleeplessness plays a prominent part is, as we know, very great. For these reasons then, and more might be adduced, I have thought that a few remarks on this subject might be considered of interest, and tend to elicit some valuable observations from the members of the Association.

The causes of sleep may be divided into proximate and remote, and it is of the former that I would wish to speak here. It is, in fact, to the physiology of the function that I would invite your attention. On this point, you are aware, a great difference of opinion exists, or at least did exist till Dr. Durham published his paper in the "Guy's Hospital Reports" for the year 1860. Till this paper appeared the most generally accepted opinion amongst physiologists was, that the direct cause of sleep arose from venous congestion and consequent pressure. This idea has been strongly opposed in the paper to which I allude, and the author has advanced a number of arguments, and a series of experiments on some of the lower animals, chiefly dogs, by which he considers the question is set at rest, and that so far from there being venous pressure present during sleep, there is in reality a diminution of blood in the brain. I must say, for myself, that though I have read this paper attentively, and more than once, I have not been able to arrive at the same conclusion as the author. It is quite true that others, and conspicuously

amongst these Hammond, in his valuable and interesting work on "Sleep and its Derangements," have advanced similar opinions. Still there are others who hold opposite views, and of these I would particularly mention Cappie, whose essay on the subject is well worthy of perusal. My own impressions, I have just said, are opposed to those which are now generally held, and I wish here to state my reasons for venturing to differ from such men as Durham and Hammond.

Before stating my own views it will not be considered out of place if I give the briefest sketch of those experiments and thoughts advanced by these gentlemen, and on which they found their opinion, that during healthy sleep a diminished quantity of blood circulates through the brain. Having trephined a dog, at the time under the influence of chloroform, Durham observed the large veins of the surface of the pia mater were distended, and the smaller were full of dark-coloured blood, and the longer the chloroform was used the greater was the congestion. As the effects of this agent passed off, the animal sank into a natural sleep, and then the condition of the brain was very materially changed. Its surface became pale, and sank down below the level of the bone, the veins ceased to be distended, and many which had been full of dark blood could no longer be distinguished. When the animal was roused the surface of the brain became suffused with a red blush, and it ascended into the opening through the skull. As the mental excitement increased, the brain became more and more turgid with blood, and innumerable vessels sprang into sight. The circulation also increased in rapidity. After being fed, the animal fell asleep, and the brain again became contracted and pale. In all these observations the contrast between the two conditions was exceedingly well marked. To this account, which I have taken nearly *verbatim* from Dr. Durham, it need only be added that, to obviate any possible effects·due to atmospheric pressure, watch-glasses were applied to the opening in the skull, and securely cemented to the edges with Canada balsam.

Many experiments of a similar kind were performed, and led to the same results; and, from the whole, the author arrived at the following conclusions, which I give *verbatim* :—

1. Pressure of distended veins upon the brain is not the cause of sleep; for during sleep the veins are not distended, and when they are, symptoms and appearances arise which differ from those which occur in sleep.

2. During sleep the brain is in a comparatively bloodless condition, and the blood in the vessels is not only diminished in quantity, but moves with diminished rapidity.

3. The condition of the cerebral circulation during sleep is, from physical causes, that which is most favourable to nutrition of the brain tissue; and, on the other hand, the condition which prevails during

waking is associated with mental activity, because it is that which is most favourable to oxidation of the brain substance, and to various changes in its chemical constitution.

4. The blood which is derived from the brain during sleep is distributed to the alimentary and excretory organs.

5. Whatever increases the activity of the cerebral circulation tends to preserve wakefulness, and whatever decreases the activity of the cerebral circulation, and, at the same time, is not inconsistent with the general health of the body, tends to induce and favour sleep.

In these conclusions, I may say, Hammond coincides—this gentleman, as he states, having made similar experiments both before and after the publication of Durham's paper, and having arrived at the same conclusions. In the course of the following remarks there will be occasion to notice some of the points which Hammond states in his work.

It must be admitted that the facts and arguments advanced by these two writers seem to determine the question at issue; and yet, as already stated, I find a very great difficulty in accepting these conclusions, and I now proceed to state my reasons for at least doubting them.

When I first read Durham's paper, which, as you know, appeared in 1860, it brought to my mind a case which occurred several years previously, and with which I had very much to do. It was briefly as follows :—A man of middle age, whilst drunk, had his skull severely burnt at a lime-kiln, where he had fallen asleep. In a short time very urgent symptoms of pressure appeared, and he was trephined over the centre of the parietal bone. Subsequently the opening was enlarged in one direction, and the result of the whole was to cause an opening of considerable size, and in shape like a comet. The brain was quite exposed, the dura mater having been removed to allow free vent to the pus. Whilst in this state I had the dressing of the case for several weeks, and plenty of opportunities for seeing the patient both asleep and awake, and I never saw him asleep that I did not observe marked congestion of the vessels, more particularly what seemed to be the veins, whilst all the vessels visible assumed a dark hue ; and when the patient awoke, it was very interesting to observe, first, an increase in the rapidity of the circulation, and then a change in the colour of the blood, and it sometimes seemed as if even new vessels, or at least some not seen before, had made their appearance.

As I interpreted this case, it seemed to me to bear out the views held on the subject at that time, and so I continued to think of it till Durham's paper appeared, which naturally raised the question—Was the congestive hypothesis—as it may for brevity be called—true, or was it now essential that the views held by so many, and so long, were to be entirely reversed. Then, in 1869, appeared Hammond's work, confirming Durham's opinions still further. To these views, as already stated,

there seem to me to be very strong objections, and I may proceed to state my reasons for differing from these gentlemen, and will now take up each of Durham's conclusions *seriatim.*

The first is, that "Pressure of distended veins upon the brain is not the cause of sleep, for during sleep the veins are not distended; " and the second may be joined to this, being a kind of corollary from the first. It is, that "During sleep the brain is in a comparatively bloodless condition." Now, to this statement I would bring forward the case already detailed, and though Durham's and Hammond's experiments contradict it, I see no reason to doubt the evidence of my own senses. But there is another very important point involved in Durham's proposition—viz., that pressure on the brain's substance is not the cause of sleep. To this I must demur, for I believe it to be at least one of the causes. Further on I shall state my own views on the matter. I would here remind my hearers of an experiment which has been so often repeated as to leave no doubt of its truth—I mean when the brain is exposed subjecting it to a slight pressure. It is known that, by doing so, consciousness is at once lost; and indeed I may state I saw this done—unadvisedly, I admit—in the case to which I have reverted, and on one occasion, the pressure being carried a little too far, a violent convulsion was the result. It must be admitted that this kind of pressure is not Nature's pressure, still it is pressure, and produces a state which, to say the least of it, is very like sleep. But there are other objections to Durham's conclusion, for whilst he states that the brain is in a comparatively bloodless condition during sleep, he, at the same time, dwells on the necessity of the absence of the blood being supplied by the presence of the spinal fluid— hence, as I understand him, pressure must be exercised during sleep, even though that pressure be caused by spinal fluid. This view of the question I take to be quite inconsistent with the ideas already expressed, for, according to Durham and Hammond, the brain sinks in its case during sleep; but how or why the brain should alter at all, when, at the same time, the absence of the blood is supplied by spinal fluid, I must leave to others to explain. To myself it would seem that it ought to remain the same.

But if we suppose for a moment that the views I combat are correct, other grave objections may be brought against them; for we are told that sleep is the period during which the nutrition of the organ goes on actively, and its tired-out functions restored. Whilst granting that sleep is " Nature's sweet restorer," I am quite at a loss to understand how it can go on in such a state of the organ as has been described—that is, with a comparatively bloodless condition, and, at the same time, an equivalent of spinal fluid to supply the absence of the blood. How, I ask, is nutrition to go on under such circumstances? Can it be possible that the nutrition of this organ is carried on so very differently from

what obtains elsewhere; and can we entertain the idea that a diminished supply of blood is the method Nature takes to restore what has been lost? This would surely be contrary to every known principle in physiology, and, to my mind, would alone be sufficient to prevent me accepting the views which have been advanced.

Still further, it is not possible to speak of sleep and its causes without referring to one of what may be called its modifications—I refer to dreaming, which most probably all whom I address have experienced at one period or other. Now, does this state square with the hypothesis of which I have so often spoken? Dreams, I take it, are, to all intents and purposes, mental acts, and strange and grotesque though they be, cannot, I believe, go on without the brain being in a somewhat similar state to what it is during our waking hours. If this be so, it is certainly hard to accept the hypothesis of Durham and Hammond, which makes the brain, during sleep, to be in a comparatively bloodless state—in a state, in fact, in which dreams could not occur; or, if they did, it could only be by the spinal fluid being displaced, and its place supplied by the return of blood. This alternation, then, of the kind of fluid circulating through the brain which we must accept to account for dreaming, seems to me hard to receive. We know that we may sleep and dream, and dream and sleep, with great rapidity, and this often in the same night. A dream, too, can be dispelled in an instant by a touch or a word or the instant we awake. This very rapidity of action between sleeping, dreaming, and waking, is, I repeat, hard if not impossible to reconcile with the hypothesis. And speaking of the changes which go on, I would observe on the rapidity with which we awake. In my own person, and I doubt not in may others, the act seems instantaneous. In some, it must be admitted, it is a slower process. But Durham and Hammond assume it is so in all, which is certainly contrary to my own experience, and they will have to explain why it is or seems to be instantaneous in any.

Before proceeding to state what I believe to be the proximate causes of sleep, I would wish to notice some points which are mentioned in Hammond's work, and have a direct bearing on this subject. Thus, at page 33, the author states that the effects of chloroform and ether on the brain are opposed the one to the other; that chloroform produces congestion of the vessels of the brain, and a protrusion, as it were, of the organ through the artificial opening; whilst ether causes the very opposite—that is, a sinking of the organ in its case. I read this statement, I confess, with some surprise. Even allowing that ether is a much more powerful stimulant than chloroform, it is hard to believe the effects of the two agents on the brain to be so very much opposed the one to the other. It is not easy either to see how the most stimulant of the two should be the one that causes the depression of the brain. So, however, it is stated in Hammond's work. But statements of this kind, I felt,

could not be met by any opinions of my own, no matter how strong they were. Facts, and facts alone, could answer the purpose. Now, have we any facts bearing on the matter? We have, for my recollection brought to my mind what is given in Snow's book. It, as you know, contains some fifty cases where death occurred under the use of chloroform. Out of these, in nearly thirty cases examined, a *post mortem* disclosed the fact that the cases where marked congestion of the brain existed were just a shade over half. For all intents and purposes the cases may be looked upon as even—that is, there were as many with congestion of the brain as without it. Now here we have Snow's statement, who had no theory to support, and I need scarcely say that it affects seriously the statement of Hammond. How the latter overlooked it is not easy to explain; for Snow's work appeared long before Hammond's. To myself it seems to shake the entire of Hammond's argument, and makes me slow in accepting what he has advanced.

There is, too, another point which Hammond has stated in reference to the state of the fontanelle in infants, and which he advances as an argument on his side of the question. His words are:—"I observed that in young infants the portion of scalp covering the anterior fontanelle was always depressed during sleep, and elevated during wakefulness" (p. 38). On reading this statement it struck me at once that I had the means of ascertaining its accuracy, and in a way that could scarcely be questioned; and through the kindness of Dr. Johnston, then Master of the Rotunda Hospital, and subsequently of Dr. Atthill, I followed out a series of observations extending over nearly two years. The result of this inquiry has satisfied me that Hammond's statement is erroneous. But I do not go so far as to state that the contrary is the fact. The truth is, that the results of my inquiry were negative—that is, whether the infants were sleeping or waking no appreciable difference could be observed; and I suppose my hearers will be surprised to hear that in only two instances out of a very large number could I detect a distinct elevation of the fontanelle, and both these infants were crying lustily. In ordinary crying many cases occurred where there was no appreciable elevation of the fontanelle. As my own evidence in this matter might be questioned, I asked Dr. Hart, now assistant in the hospital, to make some observations, and he, without knowing my conclusions, has arrived at exactly the same. So that, as far at least as Irish infants are concerned, the fontanelle exhibits no change, whether they be sleeping or waking. Here, then, is a second point, and one, too, of much consequence, in which Hammond's statements cannot be accepted.

It is time, however, that I should now state my own opinions as to the cause or causes of sleep; and, as I cannot accept the conclusions of Durham and Hammond, at least describe those at which I myself have arrived. And here two remarks of a general character may be made.

In the first place, I never believed, as some have done, that the amount of blood or fluid circulating in the brain was an unvarying quantity. I had the strongest convictions that it did vary from time to time, and it did not require the conclusive work of Burrows to satisfy me on that point. The mistake on this point arose, I apprehend, from assuming that the bony skull was a complete sphere, which it most assuredly is not. There are too many fissures and openings to entertain such an idea. It may be allowed, however, that the variations in the amount of fluid in the brain can be but small. But this is a very different matter from saying there is none.

The second point I would observe is, that sleep is not the simple function which writers on the subject would lead one to suppose. So far from this being the case, I believe it to be a complex function, and that no single change in our frames will induce it. Now, this I take to be an important view of the question. For, after reading Durham's paper and Hammond's work, especially the latter, the reader would almost be led to conclude that sleep depended solely on the quantity of blood on the brain being lessened. I believe this is an erroneous view to take of the matter. Surely we cannot consider the other changes which go on in our frames to be immaterial to the healthy performance of the function—I allude more particularly to the fall in the pulse, and the number of the respirations. We all know how a rapid pulse and breathing interfere or prevent sleep; and should sleep arise under the circumstances—that is, when both the breathing and pulse are rapid, we must have all observed the marked contrast which exists between the sleeping and waking moments. This is particularly the case with children who are sick, where the fall in the number of beats in the pulse and the respirations is exceedingly marked; and, on the contrary, the instant they awake, both pulse and breathing resume their unusual quickness. The conclusion I arrive at from these facts—and, as stated before, all must have observed them—is that, besides any change which may take place in the brain during sleep, the lowering of both the number of respirations and beats of the pulse are essential parts of the function, without which it cannot normally go on. This lowering of the pulse and respiration, it may be observed in passing, has, as is well known, a marked effect on the brain. For at each inspiration the organ subsides considerably, whilst with each beat of the heart it is distinctly raised; but the result is that, with both pulse and breathing slowed, the brain must be much more quiescent during our sleeping than our waking hours; and this state seems to me to go far in explaining what Durham and Hammond have stated about the subsidence of the organ during sleep. The state is one of comparative quiescence; but this they have described as if the brain had shrunk, and the quantity of blood circulating through it diminished, which I consider is not the case. That in the experiments detailed by Durham and

Hammond the brain was seen to subside, I do not for a moment doubt. The vessels were first seen to be turgid with black blood; but, as the effects of the chloroform passed off, this state would of necessity diminish. It was clearly a state of disease, and as the cause was removed, so would the consequences, and these must have been a subsidence of the brain, and, in a degree, a paler colour of the vessels, just such as was described. But it by no means follows that this subsidence went beyond a certain point; for then the pressure, so essential to the healthy function of the brain, and so strongly argued for by Durham himself, would have been lost; and in keeping with this view of the matter is the fact stated, that after the engorgement of the brain subsided the animal fell into a natural sleep.

But are there any other direct causes which promote sleep besides those adduced? I believe there are, and though they may not have the same foundation in fact as those just spoken of, they yet seem to me to be in the highest degree probable. Thus, I believe, the quality of the blood circulating through the brain during sleep is of an inferior quality to what it is during our waking hours. There would appear to be more carbonic acid in it, this being most probably due to the altered breathing. We know that all living nature seeks shelter during sleep; and this must have a tendency to alter the blood somewhat. The movement may be slight, but it is in the direction of which I speak. I might instance the bird putting its head under its wing; and, amongst ourselves, the number who keep their heads under the bedclothes is much greater than any who have not paid attention to the point would suppose. This is particularly the case with children; and indeed it has often been a matter of surprise to me, both in adults and children, how they managed to carry on the breathing at all, their heads were so covered up. That many could not bear this is certain—for sleep is just like any other function, and varies with the individual. But that many cover up their heads is equally certain. And, as bearing on this point, I may allude to the effect of certain agents in causing sleep to be much more profound than it would otherwise be. Thus the moderate use of any strong drink at night leads to this—and I suppose there is none with which most of us are more familiar than whiskey—which, by the additional quantity of carbonic acid thrown into the blood, produces such marked effects; and should the quantity be such as to lead to a fatal result, we know that intense congestion of the vessels of the brain will be found; from which, I infer, that a more moderate use of it would cause less congestion of the organ, and yet cause or allow of sleep—contrary to the hypothesis from which I venture to differ.

Lastly, position has very much to say to healthy sleep; and yet it is strange how very little prominence is given to it by authors. For myself, I believe far more is gained by it than mere relaxation of our muscles'

important though that may be. I consider that such a circulation through the brain as is directly conducive to sleep is gained by the horizontal position, and this with no strain on the system, or rather, I should say, with a diminished one; for both the breathing and respiration are lowered, as already stated. But this is met, I believe, by the position in sleep, where it has been matter of observation to me that the soundest sleepers are those that lie flattest; and, *vice versâ*, those easiest wakened are those who use one, or even two, pillows, in addition to the bolster ; but if the hypothesis I question were true this should not be the case.

It was my intention to have made some observations on sleep in connexion with disease, for the derangements of the function are, we know, very varied and important. But my remarks have extended to such a length that I must, for the present, conclude. Before doing so, I may just state that I believe sleep to be a modification of coma, brought on, however, by natural causes, which have been already glanced at, and, as the poet has it, of " pure digestion bred." I believe too that this function is induced by no single cause, but by several combined ; and this view of the subject seems to me to meet all the rquirements of the case better than any other with which I am at present acquainted.

DR. FOOT said Dr. Kennedy seemed to have merely touched on a current opinion as to the cause of sleep, which was quite different from the view attributing it to either anæmia or hyperæmia, and which set it down as due to an accumulation of carbonic acid in the blood. The view was that of Sommer, and was founded upon Pettenkofer's and Voit's researches on the excretion of carbonic acid. This explained the periodicity of sleep, and the known impossibility of doing without it. During waking hours the formation of carbonic acid in the blood was always in excess of the excretion of it. It increased in proportion to the amount of work done, until the quantity rose to a poisoning point, and then the brain became affected, and sleep either troubled or tranquil should take place. In harmony with this view were the stimulating and wakening effect of tea and coffee, which were known to influence the formation of carbonic acid in the blood ; the absence of sleep in cases of derangement when the brain appeared to be anæsthetic to the influence of carbonic acid, and also the great sleeping habits of heavy eaters whose blood was constantly loaded with carbonic acid and its compounds.

DR. ASHE said that some years ago he treated a case of intense insomnia in a child of three years old. For more than four nights the child had not slept, and had got into a state of the most intense excitability, screaming and crying out for sleep. The child's stomach turned and some particles of meat were thrown up, which the mother assured

him had been eaten two days before, and which were in a perfectly undigested state. Thinking that there might be some congestion about the eighth pair of nerves he applied a strong mustard blister over the back of the child's head and top of the neck. At first the child cried out violently from the pain, but in ten minutes fell into a profound sleep, and made a capital recovery. This rather led him to think that congestion of the vessels of the brain was the cause of want of sleep. In other cases' of intense insomnia, associated with insanity and mania, he had produced sleep by shaving the head and applying lint well wet and allowing the moisture to evaporate. On examining the brains of persons who had died of insanity or mania he generally found they presented minute red points, and had sometimes a purplish tint due to intense vascularity of the minute vessels, and going to show that it was a state of congestion which had produced want of sleep.

DR. DOYLE asked had Dr. Kennedy made any observations as to the lymphatic system in the waking and the sleeping states respectively.

DR. LYONS remarked that several questions not at all of a co-ordinate character were involved in Dr. Kennedy's paper. He could not at all accept the view Dr. Kennedy seemed to wish to lay down—that coma and sleep were necessary correlative conditions. He thought they were not by any means convertible terms. Nor, again, could they properly put in the same parallel lines with sleep the conditions induced by such agents as chloroform and ether. No doubt in fever and other diseases insomnia was a most troublesome condition and was even dangerous, for when it was impossible to produce sleep death from exhaustion often ensued. He did not think there was anything in the fact to bear out the assertion that the presence or absence of carbonic acid in the blood had anything to do with sleep.

DR. KENNEDY, in reply, said that he had made no observations as to the state of the lymphatics. He had not meant to assert any natural identity between the phenomena produced by chloroform and ether and that of sleep. His view was that sleep was a modification of coma, and he thought there was a great deal to be said in its favour. Magendie had shown that a very slight degree of pressure on the brain was sufficient to deprive an animal of consciousness, and it produced a condition excessively like sleep, and that the moment the pressure was removed the animal wakened up again.

The Society then adjourned.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.

THIRTY-NINTH ANNUAL SESSION.

Saturday, April 14th, 1877.

THOMAS DARBY, F.R.C.S.I., President, in the Chair.

Uterine Hand-Spray.

DR. ATTHILL.—Before reading my paper for this evening I wish to exhibit this apparatus, which has been sent to me by Dr. Bernard, of Londonderry. It is a uterine hand-spray for injecting perchloride of iron. Dr. Bernard's directions for its use are as follows :—" It is intended to be worn on the first phalanges of the left hand, the holes, of course, on the back. When you are drawing it on, insert the fingers from above downwards between the branches of the tube, taking care to have the central one between the middle and the ring fingers. When on the hand the tubes will be next the palm and between the fingers, thereby precluding the possibility of any hard uneven substance coming in contact with the interior of the uterus. It can easily be fed through the medium of an ordinary elastic enema syringe. The contrivance will, I think, speak for itself, and I wish only to remark how much more directly than the ordinary mode of injecting for *post partum* hæmorrhage this must, by aspersion and apart from force, bring astringent solutions into immediate contact with the uterine surface while coagula are being removed, thus forestalling a fresh outburst." Weiss and Son, Strand, London, are the makers.

The PRESIDENT.—Gentlemen, this is a very simple and ingenious apparatus, and well adapted for the purpose for which it is intended.

DR. ATTHILL.—There is no doubt that the ingenuity of this apparatus reflects great credit upon its inventor. What results will ensue from its use in actual practice I am as yet unable to say.

Vesical Calculus in the Female.

DR. KIDD.—I beg to exhibit a calculus which I extracted from a female bladder. This disease is sufficiently rare to make an example of it worthy of the attention of the Society. Early in the present winter a lady came to Dublin, and put herself under my care, complaining of irritation of the bladder. On examination, I found that there was a calculus in the bladder. With the assistance of Mr. Porter I removed it. It is soft and friable, and consists of phosphate of ammonia.

The patient having been put fully under the influence of chloroform by Dr. Macnamara, I drew out a portion of the calculus with a forceps. The remaining portions were washed out afterwards with a catheter, and were passed the following day through the urethra. The lady recovered perfectly from the effects of the operation, but very soon afterwards manifested fresh symptoms of irritation of the bladder; but, after the most careful examination, I could not detect any further calculus in it. She had been for more than a year suffering from the disease; a large quantity of mucus was coming away, and we thought it was inflammation of the mucous membrane that was keeping up the irritation. She returned to town about six weeks ago, and I discovered that there was another very much larger calculus in the bladder. About a fortnight ago I removed it in the same manner as the other, namely, by passing a forceps in through the urethra, and drawing out piecemeal as much as I could, and picking out the rest with my finger. Nevertheless, we were quite conscious that some portions still remained behind. A few days afterwards the mass that you see was passed through the urethra. There was an interval of about three months between the two operations. On the third day after the removal of the mass to which I last alluded, this other mass was extruded during the passing of water. The relief the lady experienced after the second operation was very much more marked than that which followed the first; but still the symptoms had returned so rapidly after the first operation, and as we were under the impression that some portions of calculus were still in the bladder, we did not think it prudent to persevere in our efforts. Nothing could be felt with the sound, and I dilated the urethra, and explored the whole bladder completely with my finger, and thus satisfied myself that no further stone was remaining. That I did about a fortnight after the second mass was removed. We felt a good deal of apprehension as to whether the dilatation of the urethra would not be followed by incontinence of urine; but, although I had my finger in the bladder twelve times after the first operation, and Mr. Porter and I both explored the bladder on the second occasion, the lady never for a moment had incontinence of urine. All her urinary symptoms have now disappeared.

Vesical Calculus in the Female.

Dr. MORE MADDEN—I beg leave to show a stone which I removed from the bladder of a lady the other day. She was sent up to me from the country by my friend, Dr. O'Hanlon, of Castlecomer, suffering from ulceration of the os and cervix uteri of so grave a character that we apprehended malignant disease of the uterus. There was extensive ulceration, a good deal of hæmorrhage, and very fœtid discharge. The uterus was greatly enlarged. She was treated in the ordinary way with nitric acid, subsequently by swabbing out the uterine cavity with carbolised

glycerine, and the ulceration healed, and the uterus regained its normal size. After some time she informed me that for the past three years she had been suffering from a difficulty of passing water, which she could only do by lying on her right side, and placing a vessel under her. I had the advantage of a consultation with Dr. Kidd, and we at once detected a stone in the bladder. We resolved to remove it, and proceeded to do so in a day or two afterwards. Dr. Ormsby applied ether with his apparatus, which proved very successful and useful. With the assistance of Dr. Kidd I succeeded in removing this stone by the aid of the ordinary forceps. Some little difficulty resulted from the stone having been at first caught transversely, but ultimately it was extracted. For about twenty-four hours afterwards the patient suffered from incontinence of urine; but gradually the power of retaining water came back, and at present she suffers from none of her former symptoms, with the exception of a slight scalding sensation when passing water. The stone, which weighs 125 grains, consists of oxalate of lime, and must have given a great deal of irritation on both sides.

The PRESIDENT.—A great many years ago a woman brought to me her daughter, aged seven years, who, she said, had great irritation when passing water. On examination I found two very peculiar circumstances connected with the case. One was that the child had a cribriform hymen, and the other was that there was a rough oxalate of lime calculus, about half the size of that shown by Dr. More Madden, sticking in and resting on a fleshy band which passed from behind forwards across the orifice of the urethra. I divided this band, and removed the stone (something larger than a large pea), without any trouble. I have met with seven or eight cases of red sand passed by little girls, but this is the only case of calculus I ever met with in the female. Possibly it is the facility with which small calculi can pass through the short female urethra, while still small, which renders stone so rare in women.

On Transfusion in Post Partum Hæmorrhage. By LOMBE ATTHILL, M.D., Master of the Rotunda Hospital, Dublin.

ALTHOUGH _post partum_ hæmorrhage is of common occurrence, deaths from this cause are now very infrequent. This fortunate result is, doubtless, due to the fact that the causes favouring or producing hæmorrhage after the birth of the child are now in general well understood, and the state of the uterus during the third stage of labour being carefully watched, hæmorrhage is either averted, or on its occurrence promptly arrested by the use of efficient means. Still deaths from hæmorrhage do from time to time occur, and, in spite of our boasted knowledge, will, I fear, continue to do so. As an example, I give the details of the following case which terminated fatally, notwithstanding that every care was

taken of the patient during labour, and every means employed to save life which experience could dictate.

A. K., aged thirty, was admitted into the Rotunda Hospital on Tuesday the 6th February, at 1.30 a.m. She then stated that this was her eighth pregnancy; that she had come to her full time; that three days previously, namely, on the 3rd of February, about 9 p.m. the waters had escaped, and that since then she had suffered from slight pains. The patient was aged-looking and anæmic; and though she stated her age to be thirty, her appearance was that of a woman of forty; still she complained of nothing except of slight pain in her back. On a vaginal examination (9 a.m., Tuesday, February 6th), the os was found to be soft and patulous, admitting the point of the finger easily. The presentation was so high as barely to be reached by the finger, and could not be made out with certainty. The pulse was quiet, the tongue clean, and the patient, thinking her labour had not commenced, wished to go out. This she was advised not to do. She had no pains during the day and slept well that night. During the day following (Wednesday) labour advanced slightly. In the evening the cervix had disappeared, and the head could be made out presenting. At 10 p.m. she was ordered a draught containing 20 grains of hydrate of chloral and 20 minims of tincture of opium. On Thursday morning, February 8th, at 7 a.m., true labour pains set in; at 2 p.m. the os uteri was nearly dilated, the lips were thick, and there was a very large caput succedaneum.

Labour now advanced rapidly, and at 4 p.m., the child was born. After an interval of twenty minutes, during which steady pressure with the hand was kept up on the fundus, the placenta was expelled entire. No hæmorrhage occurred, and the uterus being firmly contracted, the binder was applied. After the lapse of about half an hour, however, a stream of blood was observed trickling from the vulva, slight in quantity, but flowing continuously. The binder was consequently loosened, and the state of the uterus carefully examined. It was found to be fairly contracted, and the binder was reapplied. The oozing, however, continued. Dr. Hart was now summoned. He injected ergot hypodermically, applied firm pressure, and a clot was expelled. The little stream of blood, however, still continuing to trickle down, he injected cold water into the uterus, without obtaining any result; the blood still trickled down, just as before. The patient's condition now became alarming; her pulse could hardly be felt, and she complained of feeling very weak. She was in this state when I saw her (5.45 p.m.), and, without any delay, we proceeded to inject a solution of the perchloride of iron, passing the tube up to the fundus of the uterus, and injecting about six ounces of a solution of the strength of one ounce of the strong liquor to four of water. This at once arrested the hæmorrhage, and no further loss occurred, though a watery discharge, small in

quantity, was perceptible on the sheet. The patient's condition now improved, the pulse returned to the wrists, the feet and body were warm, and she expressed herself as feeling comfortable. There had not been any vomiting. Hot punch and beef tea were freely given and were retained. This satisfactory state, however, did not last long. On return- ing to the ward, after an absence of about twenty minutes, I found her almost in a state of collapse, from which the hypodermic injection of ether roused her but slightly. Seeing that her life must speedily become extinct, unless the vital powers could be invigorated, I decided on trying transfusion, and sent for Dr. R. M'Donnell, who has, on several occasions, so successfully performed this operation, to aid us. He came promptly, and at about 7.45 p.m. the process was commenced—fifteen ounces of blood being willingly afforded by Mr. Gage, one of the intern pupils of the hospital. During the interval which elapsed, while the preparations were being made, the patient's condition had not altered, excepting in this that she vomited copiously and became cold. Still the case did not appear by any means hopeless. The vein was exposed by Dr. M'Donnell without much difficulty, and though flaccid was not absolutely empty of blood. The point of the tube, which conducted the defibrinated blood from the pipe, was quickly inserted into it, and the process of transfusion at once pro- ceeded with. The blood entered the patient's vein freely, and almost entirely by its own gravity. So far, everything was satisfactory, but the favourable results we anticipated did not occur. The pulse did not return to the wrist, and the patient, instead of expressing any sense of improvement, became very restless and complained of great distress and of pain in her chest. The whole quantity of blood contained in the pipette slowly passed into the patient's system, and we continued to hope that, after a short interval, its beneficial effects would become visible, but in this we were disappointed. The restlessness and jactatation increased, and the breathing became shorter and shallower. Ether was again injected hypodermically with transient benefit, and brandy and water administered in small quantities, but in vain. She gradually sank, and died at 10 p.m., six hours after the birth of her child—two after the transfusion had been effected.[a]

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[a] The following are the details of the operation, as reported by Mr. Walter Ryan, who had charge of the case :—Dr. M'Donnell's transfusion apparatus consists of a glass cylinder capable of containing about 14 ozs.; this at one end is drawn into a tube which tapers to a point ; the other end forms a larger tube, expanding into a bell- shaped mouthpiece. By means of this mouthpiece the cylinder is readily filled, the blood being sucked into it by the operator; when full, the thumb placed on the mouthpiece prevents the escape of the fluid from the lower tube. To this latter a piece of india-rubber tubing about the thickness of a quill is attached, which at its centre expands into a small bag about the size of a walnut ; pressure on this imparts a pulsatory motion to the flow of blood through the tube. To the other extremity is attached a silver canula, with a small aperture about ¼ inch from its end. Through

A *post mortem* examination was made twelve hours after death, by Dr. George Duffey, pathologist to the hospital, of which the following is a note :—

" On laying open the abdomen, the uterus was found to reach nearly to the umbilicus. There was slight vascularity of the peritoneum. The uterine walls were fully one inch in thickness, not infiltrated with either blood or serum, and quite firm. The cavity of the uterus contained a large quantity of black fibrinous shreds and coagula, some of which were firmly adherent to the mucous membrane. On the posterior wall of the fundus was an elevated sessile mass, about the size of a crown piece. It could only with difficulty be separated from its attachment, and resembled an altered blood-clot or fibrinous mass, which had partly undergone fatty degeneration. The right lung was healthy, with the exception of slight and easily broken-down pleural adhesions. The left lung was so extremely adherent that it was impossible to remove the lower lobe without lacerating it. This portion of the lung was of a bright, glistening, red colour, studded with minute black points. It was non-crepitant. No plug was found in any of the larger branches of the pulmonary artery leading to it. There was a large quantity of fat on the surface of the heart. Its cavities were empty, and their walls extremely pale and flabby. To the naked eye the cardiac muscle seemed, to be in a state of fatty degeneration. The walls of the right ventricle were markedly attenuated."

This case raises several questions of interest and importance. Among these are the following, to which I propose specially to direct attention :—

1. To what causes are we to attribute the hæmorrhage in this case, every care having been taken to prevent its occurrence, the uterus having

this hole, when the *point* of the canula has been introduced into the vein, any contained air escapes, and when the blood is also seen to escape through it, the canula is pushed into the vein without danger of admitting air into it. Dr. M'Donnell having exposed the vein by a transverse incision, passed a tenaculum beneath it, and, pinching up the external coat with a forceps, carefully slit it up. The canula was now introduced as far as the aperture previously mentioned, and the cylinder being raised above the patient's arm, the blood was allowed to flow by its own gravity, and when it appeared at the hole in the canula, the latter was pushed well into the vein. The blood now flowed steadily into the arm, being occasionally hastened by the operator blowing through the mouthpiece above. The operation lasted about ten minutes. When the cylinder was almost empty, the thumb was placed on the opening in the vein, the canula withdrawn, and a pad and roller applied. I should mention that during the operation the patient's respiration became heavy and of a groaning character, and from the time of the operation till her death, a period of two hours, she was extremely restless and complaining of great thirst. Respirations were 36 per minute. Subsequent to the operation no difference was perceptible in the pulse except after the injection of 25 min. of sulphuric ether, when for a moment it became stronger, but could at no time be counted. A teaspoonful of brandy and water was given every five minutes. The patient, however, never rallied, and expired quietly at 10 p.m.

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contracted firmly after the expulsion of the placenta, and the relaxation of that organ, which occurred subsequently, being only to a limited degree?

2. Was the injection of the styptic delayed too long?

3. To what was the distress which followed the transfusion due?

4. Was the transfusion not only a failure, so far as its beneficial effects were concerned, but may it not have had a positively injurious effect?

5. And if so, how can such for the future be guarded against in similar cases?

1. In reference to the first point, the previous history of this poor woman is of much importance. She had given birth to seven children, five of whom were alive. Shortly before her admission into hospital her husband knocked down and injured a child while driving a van. He was arrested and committed to gaol, she and her five children being thus deprived of the means of support. For some days previous to her admission she had been without proper nourishment. Whether or not as a result of the shock caused by her husband's arrest was not ascertained, but certain it is that the membranes ruptured prematurely, and that the waters drained away before she was admitted into hospital. As a consequence, the first stage of labour was very tedious, and was protracted over several days, during which she brooded over the condition of her children, the youngest not two years old, absolutely destitute of food, and without any one to care them. Here, I think, we have a clue to the cause of the hæmorrhage in this woman. Impoverished blood, due to the want of food (perhaps, also, to the existence of previous thoracic disease), and an exhausted condition of the nervous system, the result of extreme mental depression. The nerve-force was exhausted, therefore the uterus failed to continue firmly contracted, while the blood, being deficient in fibrin, did not coagulate in the uterine vessels, for, as Dr. Barnes points out, by means of the formation of clots in these vessels, "many women are rescued, to all appearance, from imminent death, after the most profuse and uncontrollable floodings."[a] The conditions I have here pointed out as being, in my opinion, the chief causes of the fatal hæmorrhage which occurred in this case, are not dwelt on in any of the systematic works on midwifery in general use. Doubtless, they are recognised by Barnes and others, but mentioned in so vague a manner as to be altogether undeserving of their real importance. The only writer who makes special reference to them is Dr. M'Clintock, who, in his admirable Annotations, appended to the new edition of "Smellie's Midwifery," edited by him, points out these as favouring the occurrence of *post partum* hæmorrhage. At pages 387-8 he says—"A third condition there is whose influence must not be altogether ignored, and that is the coagulable power of the blood itself;" and "I have frequently had occa-

[a] Lectures on Obstetric Operations, p. 455.

sion to observe that extreme mental depression, whatever may be its cause, can exert a paralysing influence upon the uterus." I may here state that, according to my experience, cases such as that I have just detailed, in which a small stream of blood trickled away constantly, though not so alarming in appearance, are far more difficult to treat than those in which profuse hæmorrhage occurs immediately after delivery.

2. That this was a case in which the injection of the perchloride of iron was justifiable, will, I think, be admitted by every impartial reader. Cold had failed to produce any effect; in fact, it had only rendered matters worse. Ergot, &c., had been administered in vain. The hand, certainly, had not to be introduced, because such a proceeding did not promise to be productive of good, for the uterus was fairly contracted, and it was quite evident that there were not any clots in it. Clearly, the stream of blood that slowly but continuously trickled from the vulva must be stayed, or life must soon ebb away, and no means for effecting this remained except the injection of a styptic. For the first time in my experience of the use of this agent, not a very limited one, it failed to save life. I ask, should it have been practised earlier ? The result, I believe, proves that it should ; but then we did not, at that time, anticipate the result; and acting on the rule, in my opinion injudiciously laid down, not to inject a styptic for the purpose of arresting hæmorrhage "*till the means usually employed for that purpose have failed,*" we in this case delayed too long. I take this opportunity of repeating, what I have on a previous occasion stated before this Society, that I have not once had cause to regret injecting the perchloride of iron in cases of *post partum* hæmorrhage. While this is the second occasion on which I have had to regret delay in its use, it should be observed that in this case the hæmorrhage was not only arrested but that the patient rallied for a time after the injection. The styptic did its work, but the vital powers had sunk to too low an ebb for life to be saved. The conclusion forced on my mind that this patient's life might have been saved by an earlier recourse to this treatment, is the first of the practical lessons to be deduced from this case.

3. The *post mortem* examination, while failing to account positively for the distress from which this patient suffered while the transfusion was proceeding, revealed two facts of great importance—namely, the existence of disease of old standing of the base of left lung, and of a fatty condition of the heart. With respect to the former, I think it is a fair inference to suppose that notwithstanding the care taken by Dr. M'Donnell in defibrinating the blood, a minute particle of fibrin may have passed through the right heart, to become the cause of obstruction at a point where previous disease had existed, the result being infarction of the lung tissue there, and that to this the symptoms were due.

4. The condition of the heart also was most unfavourable to a successful result. This has been pointed out by Prof. Cesare Lombroso. In a memoir on Transfusion he gives the details of 41 cases in which the operation was performed, and states that in individuals with atheroma or fatty heart it is specially dangerous. His results, indeed, are not very encouraging, for out of his 41 cases in 8 the operation was injurious, and in 2 death followed; but then it should be borne in mind that it was practised in several instances on unsuitable subjects—namely, on patients suffering from various forms of chronic disease, such as leukæmia, anæmia, uræmia, and madness, and that he adds that the results were far more favourable in cases of parturient or traumatic hæmorrhage than in any others. Still I think, with the facts now before us, the symptoms exhibited by the patient being taken in connexion with the *post mortem* appearances, it can hardly be doubted that in this case death, which was previously inevitable, was accelerated by the operation.

5. The inference to be drawn from the foregoing facts is clearly this— that transfusion is not a perfectly harmless proceeding, and that we should, if possible, before undertaking it, satisfy ourselves that the patient is free from any disease of the lungs or heart, but this is not always an easy matter; we are generally called on to perform the operation on the instant, on patients of whose previous history we are most likely ignorant, and in whom, on account of her exhausted condition, a satisfactory examination of the state of the lungs and heart is nearly impossible; still the attempt should evidently be made, and the operation should not, in my opinion, be undertaken if we have reason to suspect the existence of disease of either the lungs or heart.

In conclusion, I wish to point out that the condition of the uterus, as revealed by the autopsy, altogether negatives the statements that when perchloride of iron is injected into the uterus it enters the veins; the uterus contained "black fibrinous shreds, some of which were firmly adherent to the mucous membrane," but in no place was there the slightest discoloration of the uterine walls subjacent to the mucous membrane, or any trace of iron in the uterine veins.

The PRESIDENT.—This is one of the most important cases that could possibly come before us, and I shall be happy to hear any gentleman who wishes to make observations upon it.

Dr. ROBERT M'DONNELL—I wish to say a few words on the subject of this communication; and as what I have to say forms, in fact, a part of the communication itself, it is well that it should be the forerunner of any discussion that may take place. I am much obliged to Dr. Atthill for having given me the opportunity of being here, and of looking over his paper beforehand, as it does not come upon me for the first time there are

doubts as to whether the operation of transfusion may not have been the means of accelerating the patient's death. I wish, therefore, to reply pointedly to the two observations on the paper which have been specially directed to this subject, because I consider that if they were allowed to pass unchallenged, and without a satisfactory answer, the communication would be unfavourable to the performance in future of this operation as I recommend it to be performed.

In the first place, it is said that the cause of the distress felt by the patient was the transfusion, and that distress accelerated the fatal issue. I think the best answer to be given to that, and the only answer that we can give is, that we have not perceived in other cases where the patient has been sufficiently conscious to be able to describe the symptoms experienced as the operation went on, anything which depresses the vital powers, causes distress, or seems to tend at all towards a fatal issue. One case in which I performed the operation was that of a girl who was suffering from tetanus, and dying of starvation. She was perfectly intelligent, and able to describe with the greatest precision her sensations as the blood entered the vein; and so far from speaking of distress, she described it as an agreeable, warm, revivifying feeling. She had no laboured breathing, or any distress whatever. I appeal, therefore, to cases of that kind as important in showing that the operation of throwing in defibrinated blood into the vessels is *per se* not likely to produce that kind of depression which accelerates a fatal issue. We need not lay the blame on the transfusion in such a case as that which we have heard. We have a wretched, starving woman, with a number of children starving along with her, her husband in gaol, and unable for several days to provide food for his family. I do not think, when we are brought to the bedside of a moribund creature in that condition, that it is necessary for us to seek very far for the cause of the laborious and painful sinking when she came to be *in articulo mortis*, as in point of fact she was when this operation was performed. I have seen cases of patients in a state of still greater depression who have come round again; but I admit that these were cases in the upper ranks of life, in which there was some material to work on, not cases where even a very moderate hæmorrhage (for it does not appear that the case before us was one of tremendous hæmorrhage) was enough to rob the patient of the little life she had remaining. The answer, therefore, that I give to the statement that transfusion produced the laboured breathing, pain, distress, and death of this patient—and the only satisfactory answer that can be given—is, that in other cases in which the patients described their symptoms we did not find that symptoms of that description occurred.

Another very important point alluded to in this case was the infarction of the lung. Pathologists who are aware of what occurs when infarction takes place, know that in this case there was too short a time for any-

thing in the nature of infarction arising from a clot getting in with the defibrinated blood to have taken place. Besides, the transfused blood was not only defibrinated but strained. I always have for this operation muslin ready for straining; and it having been scalded and perfectly cleaned—which is a very important part of the process—the blood was first stirred with a glass rod and then strained through the muslin. Therefore, I feel tolerably certain that under these circumstances it would have been impossible for any clot to have got in and given rise to an embolism. If that had occurred, the advantage of my mode of operating would be entirely gone; for the whole advantage of it, in my opinion, consists in its doing away with one of the greatest dangers that is to be apprehended in attempts of the sort—namely, that of embolism. At all events, every care was taken that human skill could employ in order to avoid the occurrence of embolism. The last volume of Clinical Lectures published by the Sydenham Society, contains one on the subject of Transfusion, by Dr. Lesser, a German clinical lecturer, at the close of which he speaks discouragingly of the method of putting in defibrinated blood, which, he says, can hardly be called transfusion, if by that we mean the direct passage of blood from one person to another. He says it is rather injection than transfusion of blood. I have been extremely anxious for an opportunity of replying to this, and of giving to it the same answer that I have already given to the first statement of Dr. Atthill. My answer is this—in four out of a very considerable number of cases in which I performed transfusion, the operation was brilliantly successful. In not one of these cases was there anything of ecchymosis or purpuric spots, or anything of that kind, on the body. A lady on whom I operated some years ago along with Dr. Ringland made a speedy recovery. A considerable quantity of blood—about the same as was transfused in the present case—was put in in that case. I may say that, owing to some accident, it had become partially cold—colder than I should consider it safe to use if I could help it. Yet her recovery was extremely brilliant, rapid, and prompt. She was able to be out before many days afterwards, and, I think, almost as speedily as in the case of ordinary confinements. Another case is reported by Dr. Beatty, in which the recovery was so rapid of a lady from a condition of apparent death, that Dr. Beatty spoke of it to a friend of his as being "not a recovery, but a resurrection." I never myself saw anybody so far gone and low as in that case. In all of those cases, and also in the case of tetanus in which I operated, and in which the patient lived for several days afterwards, although she did not recover, there was an absence of the symptoms pointed out in the lecture of Dr. Lesser.

In conclusion, I would say a word on the advantages of injecting defibrinated blood rather than using Dr. Aveling's instrument, in which the attempt

is made to inject blood containing fibrine. I must repeat what I said at a former meeting here, when the subject was under discussion before, that I am entirely in favour of using defibrinated blood, on both surgical and physiological grounds. Fibrine has long been supposed to be an all-important nutritive element in the blood. We now know, as Simon has long since carefully pointed out from an accumulation of observations, that fibrine is not an important element in the blood plasma, but is rather an excrementitious substance coming away from the tissues, than a source of nutriment to them. There is no more effectual means of stimulation than the introduction of the red corpuscles of the blood. They are all-important, and have the same effect as the subcutaneous injection of ether, which, we all know, is an admirable mode of treatment in such cases. Another important effect of transfusion is, that where the vessels are suddenly emptied of blood, and the heart has nothing to work on, a *point d'appui* is thus supplied to the heart. For this purpose water or saline solution would do just as well. We know in cholera that a marvellous improvement is sometimes wrought in the condition of a patient by merely increasing the quantity of fluid in the veins by throwing in a saline solution. The great advantages then of transfusion are, that it supplies at the right moment the kind of stimulus that the blood requires, and by the increase of the amount of fluid in the circulatory system gives the heart something to act on. [Dr. M'Donnell then exhibited and explained the instrument he used for the purpose of transfusion.]

DR. HENRY KENNEDY.—It appears to me that exception might be taken to the statement of Dr. Atthill, that the transfusion had caused the distress which the poor patient referred to her heart just after the operation was performed. I have seen several cases of fatal bleeding where no transfusion was performed, and in which all the patients referred to a kind of distress, or agony, in the heart's region, just as occurred in Dr. Atthill's case, and very near their death. I have witnessed this in cases of typhoid fever, ruptured aneurism, and hæmatemesis. In a very late periodical a case of what was designated pernicious anæmia is given, in which the patient rapidly went from bad to worse, and transfusion was practised with very marked benefit, for the patient revived at once, and lived several weeks afterwards. In this instance a much larger quantity of blood was used than is common. During the last visit of the cholera to London it became common to inject saline solution into the veins, in some instances to the amount of eighty ounces, and with the result of saving some of the lives of the patients, who were in complete collapse when the transfusion was performed. As regards the nature of the fluid to be injected, I may refer to the only experiment on a living animal in which I ever took part. A dog was bled from the jugular vein till it seemed to die, the animal

having been held up by the hind legs to allow all the blood to escape.
While it was in this state a quantity of tepid water was thrown in, and,
to my astonishment, the dog not only revived, but was actually able to
stagger about. It did not, however, eventually recover.

DR. M'CLINTOCK.—The admirable and instructive remarks which have
fallen from Dr. M'Donnell form a valuable supplement to Dr. Atthill's
most interesting and suggestive paper, and must be received by the mem-
bers of the Society with the utmost deference and respect. I think the
result of the operation in this case, however, is anything but encouraging
to the practice of transfusion. *A priori* we should have expected a bril-
liant result from it, considering the circumstances under which it was
performed, with every appliance and assistance at hand that science and
skill could give, the operation accomplished without any untoward
occurrence or mishap, and performed with the highest degree of dexterity
that large experience could confer. Dr. Braxton Hicks, of London, on
this question of transfusion, has published, in " Guy's Hospital Reports," a
series of cases in which transfusion was performed by himself, and the
results were most unsatisfactory. Now, although the operation is a
simple one to look at and describe, we know very well it is one that a
man cannot always perform with advantage under circumstances where
a delay of half or three-quarters of an hour may make a difference of life
and death. With regard to the recoveries that have been attributed to
the operation of transfusion, we must, I think, put them into the category
of *post hoc*. In very few instances can you say with absolute certainty
that the recoveries have been *propter hoc* after transfusion. What suggests
this doubt to my mind is, that I have seen patients recover after *post
partum* hæmorrhage where there did not seem the slightest possibility of
life being preserved. As to this case of Dr. Atthill's, I must say that it
was treated according to the most advanced principles of obstetric science.
Ergot was injected hypodermically ; a strong solution of perchloride of
iron was injected into the uterus ; sulphuric ether was also injected
hypodermically ; and, lastly, transfusion was practised in the most skilful
manner ; and yet, in spite of all this, the woman dies. There are two
points of treatment which I did not catch from the paper whether they
were used or not. One, to which, from my own experience, I attach a
great deal of importance, is, the keeping of the patient's head and
shoulders much below the level of the hips, by taking away the bolster
and pillow from under the head, and raising the foot of the bed. Pro-
fessor Lesser, of Berlin, in the paper to which Dr. M'Donnell has alluded,
discusses the other practice, which he calls autotransfusion. But you would
hardly gather from the term what is meant by it. Autotransfusion is
nothing more than putting an elastic bandage on the limbs, after the plan
of Esmarch, and thereby forcing all the blood up to the heart to assist

in keeping alive the vital spark. I have no doubt that this may be a very valuable auxiliary. A considerable quantity of blood may thus be squeezed up from the legs to where it is urgently needed to maintain the functions essential to life. Hence I think this mode of proceeding is well deserving of our attention. It is easily done, attended with little or no risk, and might often be of value in these terrible cases of *post partum* hæmorrhage.

DR. KIDD.—It sometimes happens that when a thing is described at home it is overlooked, but that if it be described by a professor in Germany or elsewhere it is at once taken cognisance of. Fully fourteen years ago Dr. Wyse, of Middleton, in the County of Cork, wrote a paper, in *The Dublin Quarterly Journal*, of which I was then the editor, in which he described this method of applying bandages to the limbs, in order to prevent the return of the blood from the brain to the limbs, and also suggested the raising of the arms and legs, in order to facilitate the return of the blood to the body. For some years past it has been the practice in the Coombe Hospital, in cases of extreme hæmorrhage, to adopt this plan to some degree. We there raise the pelvis above the level of the head, and I have no doubt that it is a very beneficial mode of treatment. I have seen a good many cases of transfusion, and I have performed the operation myself on several occasions, but I am sorry to say that I have never seen a successful case of transfusion. In every case in which I have seen it attempted I believe that the patient was too far gone, and that that was the reason it failed. The great practical lesson to be learned from what has been said to-night about transfusion, and what has been written on the subject, is that we should not be so much afraid of the operation as we have been. In the cases in which it has been tried and has succeeded, no injurious results have been recorded as directly referable to the transfusion. I have seen many cases in which the operation was hopeless, because it had been put off until too late. We have all, no doubt, seen cases such as those described by Dr. M'Clintock, in which the recovery from extreme hæmorrhage was almost miraculous, and such as no one could have anticipated. But that would not justify us in leaving a bad case of hæmorrhage alone, in the hope of such an extraordinary recovery taking place. On the contrary, I think the fact that we have established, that transfusion can be performed without any injurious result, should rather incline us to perform it much earlier than we have been in the habit of doing. I think we are all greatly indebted to Dr. M'Donnell for his remarks to-night. The subject of the use of defibrinated blood is very warmly discussed at the present day. In *The British Medical Journal* of this day are two letters, in which the use of defibrinated blood is spoken of as being contrary to all scientific principle. Dr. M'Donnell has established the opposite for us to-night, on the most

correct physiological principles, and I am very glad that we have had the matter discussed.

DR. FINUCANE.—In the spring of the year 1855, we had eight or nine weeks of frost and snow, and there were a great many cases of *post par-tum* hæmorrhage in the Coombe Hospital. Dr. Kidd, at the time, gave a solution of the matter which was reasonable—namely, that, in conse-sequence of the continuance of bad weather, numbers of working men were thrown out of employment, and their wives were consequently in a state of semi-starvation. Dr. Kidd will, no doubt, recollect that we used scarcely to be able to check the hæmorrhage in a shorter time than three or four hours, and that no bandaging, ergot, or injection ever sufficed; but that the only way of controlling the hæmorrhage was by holding the uterus for three or four hours, which was accomplished by the medical attendants relieving each other at intervals. I remember one case like that mentioned by Dr. M'Clintock, in which there seemed to be not the slightest hope of the patient's recovery. She had a weak circulation, and the effect of cold seemed to be to interfere with it and to throw extra work on the heart. There was no hope, however, of the woman's recovery. Heat was applied to the entire surface of her body; a five-gallon jar of hot water was placed in her bed; hot bottles were placed to her arms and lower limbs, and brandy was injected by the rectum. In a few minutes she fell asleep, and in half an hour afterwards awoke and her pulse had then recovered, and she asked for a drink. I dare say Dr. Kidd remembers the case very well. I was in attendance on the woman for the entire night, and she eventually recovered.

DR. ATTHILL, in reply, said :—There appears to be some slight miscon-ception as to the object of my paper. First, I wish to state that it is im-possible that any operation could have been more skilfully and successfully performed than that performed by Dr. M'Donnell, to which it relates. In every respect it was most successful. If I have not said so, in so many words in my paper, it is clearly to be inferred from it. In the next place, I wish it to be understood that I have not the slightest wish or intention of discouraging the operation of transfusion; on the contrary, I will per-form it again, either myself, or with the assistance of the skilled hands of my friend, Dr. M'Donnell. But I consider it my duty to bring forward every fact connected with the serious cases that come under my obser-vation, including the unsuccessful as well as the successful ones ; and the impression left on my mind still is that, in this particular case, the operation hastened death, not necessarily because of the nature of the operation itself, but because it was performed on a diseased person—a person with a fatty heart and a consolidated lung. If it is possible, hereafter, to diagnose the existence of these conditions in a patient in

whom transfusion is proposed to be performed, we ought, I think, to hesitate. I am well aware that it may not be possible in all cases to satisfy ourselves as to the condition of the thoracic organs, before performing transfusion; and I do not wish to discourage the operation, if the case should appear to be a suitable one. Dr. M'Clintock has alluded to the fact of my asking Dr. M'Donnell to assist me. I did so because I consider the operation frequently to be a most difficult one. I have only once performed it myself; and in the state in which the patient, who was the subject of my present paper, was, the operation required the greatest skill and nicety. Dr. Kennedy has mistaken me in supposing that I attributed the symptoms of distress to the operation *per se.* They were due to the operation only because it was performed on a diseased patient. When those symptoms appeared, I asked Dr. M'Donnell if he could account for them, and he said that he had never seen such symptoms occur before when performing transfusion. Therefore, we may fairly infer that in the previous cases in which Dr. M'Donnell performed the operation, there did not exist the conditions of a fatty heart and consolidated lungs. The same inference is to be deduced from the phenomena of Dr. M'Donnell's case of tetanus, in which the patient felt herself invigorated and comfortable after the operation; but in that case the heart and lungs were healthy. I have been asked by Dr. M'Clintock did we lower the head and shoulders of the patient. The head and shoulders were lowered to the extent of removing bolster and pillows; but we did not lift up the foot of the bed or use any form of Esmarch bandages; but his suggestion on this point is well worthy of a trial. The *post mortem* examination, which has not been alluded to in the discussion, is important, as showing that the injection of perchloride of iron is not, in the least, likely to be followed by injurious consequences. I now exhibit the uterus to the Society; and you see that there is not the slightest sign of discoloration of the uterine tissue.

Dr. M'Donnell.—Every point which has been raised by Dr. Atthill in his paper is a most legitimate one for discussion. I merely wish to add that I think his case is altogether calculated to damp our ardour about performing this operation, which I am rather an enthusiast in favour of. As to what Dr. M'Clintock has said about the difficulty of discriminating between the *post hoc* and the *propter hoc*, that is applicable to every treatment in these cases. A vast majority of these extreme cases will always be fatal, for the accident which leads to them is one of the most formidable that flesh is heir to, and we cannot hope to save many. Now and then we can hope to save one, and then it becomes a brilliant case. I can only say, in conclusion, that the operation is a difficult one. It can, with advantage, be practised on a dead body, using water; and in the cases of living patients there is a certain analogy, they are so bloodless

at the time.   One difficulty to be guarded against is that the cold some-
times makes the muscular coat of the vein contract on the eye of the in-
strument; and the latter, generally, has to be sent in with some force;
but there is no harm in that, nor any fear of the air getting in.   I believe
in the efficacy of the operation in cases suitable for it; and I can only say
that it will always give me the greatest pleasure to assist in performing it
upon the patients of any of the gentlemen here.   I have been asked by
several of them to go to the Coombe and elsewhere; and whenever they
think they have suitable cases, in which the operation can be performed
with any reasonable chance of saving the patient, I shall be delighted to
lend them my assistance among the poor, at any hour of the day or night.

The Society then adjourned.

JABORANDI IN BRIGHT'S DISEASE.

DR. BRUEN, Physician to the Philadelphia Hospital, contributes a paper
to *The Philadelphia Medical Times,* April 14, in the hopes of inducing the
profession to make use of this drug in the treatment of Bright's disease.
He gives brief notes of some of the cases in which he prescribed
jaborandi, and says he never had so favourable results from any other
drug or plan of treatment in the management of the serious and protean
symptoms of the grave disorder.   One or two drachms of jaborandi are
made into infusion with two ounces of water, and the entire amount
directed to be taken in one or two hours.   Profuse perspiration follows
the dose, which may be repeated daily, or on alternate days, as required.
Auxiliary treatment may also be employed.   [Several cases of diabetes
have, it is stated, been successfully treated with jaborandi by Dr. Laycock.
Edinburgh Med. Jour., Jan., 1876.—ED. PERISCOPE.]

CONDUCTION OF IMPRESSIONS ALONG NERVES.

IN support of the well-known theory that nerves conduct indifferently
in both directions stimuli which are applied to them in their course,
Mons. P. Bert has devised a simple and yet conclusive experiment   The
tip of the tail of a young rat was stripped of its skin for the length of
two or three centimetres, the part thus stripped was inserted in the sub-
cutaneous cellular tissue by an opening made in the animal's back, so
that its tail looked like a handle.   A few months afterwards M. Bert
divided this handle, and thus obtained two caudal trunks.   After the
section the trunk springing from the back manifests sensibility, and the
rat uttered cries when it was pinched as if it had been the real tail,
thus proving that stimuli can be conducted along nerves in a direction
opposite to what is usually considered their normal course—*Montpellier
Médical.*   S. W.

SANITARY AND METEOROLOGICAL NOTES.

Compiled by J. W. MOORE, M.D., F.K.Q.C.P.

VITAL STATISTICS

Of Eight Large Towns in Ireland, for Four Weeks ending Saturday, April 21, 1877.

Towns	Population in 1871	Births Registered	Deaths Registered	DEATHS FROM ZYMOTIC DISEASES							Annual Rate of Mortality per 1,000 Inhabitants
				Small-pox	Measles	Scarlet Fever	Diphtheria	Whooping Cough	Fever	Diarrhœa	
Dublin, -	314,666	747	867	2	19	14	—	9	20	13	35·8
Belfast, -	182,082	519	476	—	25	6	2	13	18	13	34·0
Cork, -	91,965	199	195	—	1	—	—	—	2	6	27·5
Limerick, -	44,209	99	109	—	—	3	—	—	3	2	32·3
Derry, -	30,884	77	52	—	—	—	—	—	—	1	22·0
Waterford, -	30,626	63	77	—	—	—	—	—	6	—	32·3
Galway, -	19,692	36	51	—	—	—	—	2	—	—	33·8
Sligo, -	17,285	42	45	—	—	—	—	—	1	1	33·8

Remarks.

In most of the towns the rate of mortality was uniformly very high—in Londonderry only was it rather low, while in Cork it was moderately high. In London it was 26·7 per 1,000 of the population annually, in Edinburgh 25·0, and in Glasgow 27·9. An exceptionally inclement spring is the principal cause of these high death-rates. Omitting the deaths of persons admitted into public institutions from localities outside the registration district, the rate of mortality in Dublin was still as high as 34·1 per 1·000. Zymotic affections caused 102 deaths in Dublin, fever, measles, scarlatina, and diarrhœa being the most fatal. In Belfast measles and whooping-cough continue as epidemics, while fever shows a decided increase in fatality. Whooping-cough still prevails in Galway, and fever was fatal in Waterford during the first fortnight of the period. Of the 20 fever deaths registered in Dublin, 6 were caused by typhus, 10 by enteric, and 4 by simple continued fever. Diseases of the respiratory organs caused 255 deaths, compared with 206, 151, and 162 in the three preceding periods. These deaths included 194 from bronchitis and 35 from pneumonia in the four weeks under consideration.

METEOROLOGY.

Abstract of Observations made at Dublin, Lat. 53° 20′ N., Long. 6° 15′ W., for the Month of April, 1877.

Mean Height of Barometer,	29·734 inches.
Maximal Height of Barometer (on 30th at 9 p.m.),	30·329 „
Minimal Height of Barometer (on 3rd at 9 p.m.),	28·978 „
Mean Dry-bulb Temperature,	45·9°
Mean Wet-bulb Temperature,	43·6°
Mean Dew-point Temperature,	41·0°
Mean Elastic Force (Tension) of Aqueous Vapour,	·258 inch.
Mean Humidity,	83·0 per cent.
Highest Temperature in Shade (on 2nd),	57·6°
Lowest Temperature in Shade (on 24th),	36·0°
Lowest Temperature on Grass (Radiation) (on 7th),	31·2°
Mean Amount of Cloud,	75·0 per cent.
Rainfall (on 21 days),	4·707 inches.
General Direction of Wind,	E. and S.E.

Remarks.

April was a very wintry month—cold, wet, and inclement. After the 4th the wind was almost daily in an easterly point, while the higher air-current was more southerly. Hence heavy rainfalls occurred, especially on the 3rd, 12th, 15th (1·095 inches in 24 hours), and 20th, and cold, cloudy weather was predominant. The last seven days were, however, dry. The temperature was uniformly low, although no extremes were observed, as in April, 1876, on the 13th of which month the thermometer fell to 25·4° after a snow-storm. Even in the last week the weather was cold and searching, so that the mean temperature was only 1·6° higher than that of the first week in January, while it was 1·0° below that of the first half of February. Also, throughout the month, there was no increase of warmth—the mean temperatures of the four weeks ending April 28th having been 45·8°, 46·7°, 45·5°, and 45·9° respectively. The mean temperature of the whole month was 45·9°, or some 2° below the average. A severe easterly gale was felt on the 15th and 16th. Hail fell on the 5th, 6th, 7th, and 18th. Fogs were observed on the 14th and 21st; solar halos on the 9th, 24th, and 25th; and a lunar halo on the 19th. " Festooned " clouds were seen to great advantage at 6·30 p.m. of the 22nd in an upper current from S.S.W., a cold ground-current from N.W. prevailing at the same time.

PERISCOPE.

Edited by G. F. DUFFEY, M.D., F.K.Q.C.P.

ON THE PHYSIOLOGICAL EFFECT OF ESMARCH'S BANDAGE.

DR. V. BASCH has observed the effect on the arterial tension, produced by the application of Esmarch's bandage to the lower extremity. The observations were made by means of the plethysmograph* applied to the arm. Several experiments were made on a number of different subjects, and the results are, on the whole, very concordant. During the application of the bandage the volume of the arm usually underwent a diminution of bulk, or remained unaltered, but never increased. This proves that the increase of arterial tension, if present at all, must be extremely slight. The diminution in size of the arm is probably due to a contraction of its cutaneous vessels, similar to that noticed by Mosso on electrical irritation of different parts of the surface. On the completion of the application of the bandage, an increase in the volume of the arm occurred. This cannot be caused by increase of tension, due to the expulsion of the blood from the bandaged limb, or to closing of its arteries, for experiments show that transfusion of proportionately much larger quantities of blood than that contained in one limb, or ligature of vessels as large as the common iliac, causes only a very slight and transitory increase of tension. Besides, this increase is immediate, whereas that following the application of the bandage takes some time to develop itself. It was further observed that if the leg only was bandaged, no increase of volume in the arm occurred, while bandaging of the lower part of the thigh alone produced the same effect as if the bandage was applied from the foot up. Hence it would appear that the compression of the sensitive nerves which become superficial above the knee causes a reflex contraction of the small arteries throughout the body, by acting as a stimulus to the vasomotor centre. This contraction being greatest in the vessels of the portal system, makes itself felt in the arm by an increase of blood, and, consequently, of volume. Digital compression of the saphena nerve above the knee produced the same effects as pressure by the bandage. After the removal of the bandage, the arm generally diminishes rapidly in bulk. This is due, partly to the removal of the stimulus of the vasomotor centre, partly to the temporary paralytic condition into which the previously compressed vessels fall, and which enables them to withdraw a considerable quantity of blood from the circulation. Sometimes, however, the volume of the arm remains stationary, or

* See Dublin Med. Journal. May, 1877. P. 498.

even further increases. This is attributable to a per...
tracted condition of the vascular system occurring...
(Nachwirkung) of the irritant.—*Wiener Medizinische*...
S. 88.

TREATMENT OF GRANULAR CONJUNCTIVITIS.

Dr. RISLEY, of Philadelphia, recommends a twenty per cent...
carbolic acid in glycerin in this affection. He says it...
much more satisfactory than either the nitrate of silver...
copper.—*Phil. Med. Times,* March 17.

ACETIC ACID AND THE ACETATES IN CANCEROUS AFFE...

At the Academy of Sciences, M. Eugene Curie advocates...
ment of acetic acid and the acetates in cancer of the breast...
and the stomach. He has observed under the influence of...
the pain to cease and the tumour stop in its development...
ever recognised. The acid be employs externally in...
nally be prescribes the acetate of lime or soda in doses of...
each day.—*Gaz. A... ...*

FUNCTIONAL ATAXIA AND ...

In the Philadelphia *Medical and Surgical Reporter* for...
answered an argument by Dr. Seguin of New York on the sub...
however of mixed and partial paralyses in children. Several...
cases were mentioned in which the very men the paralysis...

progressed favourably. Cases of paraplegia and ataxia of movement from peripheral irritation are sufficiently common. Dr. Hurd thinks that the ataxia in his case was purely functional, owing to irritation of the glandular branches of the dorsalis penis nerve, the speedy relief following circumcision demonstrating this. Had the phimosis not been discovered, it is natural to suppose that general paralysis with dementia would have been the ultimate result. [In connexion with the above case, we would refer to a valuable paper by Dr. Hamilton, in *The American Journal of the Medical Sciences* for October, 1876, entitled:—"Genital Irritation as a cause of Nervous Disease, with reference more particularly to a form of Muscular Spasm of the Flexors of the Lower Extremities, met with in Young Children." From a review of four cases he has seen, the writer draws the following conclusions :—That convulsive movement, more particularly of the lower extremities, the spastic contractions of the flexors of the thighs and legs, are connected, in some children, with a continued irritability of either the glans penis or the clitoris; that, as a rule, beyond irritability, there are no mental symptoms; that the exaggerated power, or hyperkinesis, is the rule; that atrophy, or actual akinesis, is exceptional; that want of coördinating power is common. Dr. Hamilton infers from Dr. Sayre's results, that the removal of the seat of irritation is followed by a cure,—ED. PERISCOPE.]

NEW PREPARATIONS AND SCIENTIFIC INVENTIONS IN RELATION TO MEDICINE, SURGERY, AND HYGIENE.

WE have examined a collection of aerated waters, prepared by Mr. W. Hay, of Hull, and which consist of potash, seltzer, lithia, and soda waters, together with lemonade, ginger-beer, and other similar beverages. So far as flavour is concerned, we can report very favourably upon these aerated beverages; we can also express an equally favourable opinion as to their freedom from those impurities which are known to exist occasionally in mineral waters. We examined them carefully for lead, but did not detect a trace of that poisonous metal. The absence of organic nitrogen and saline ammonia from all these waters, clearly proves that the water used in the preparation was free from sewage and similar impurities.

In the preparation of his ginger-beer, Mr. Hay has been especially successful. The unpleasant taste which existed in the first samples he prepared, was found by him to depend on the resinous extractive which abounds in the essence of ginger of the British Pharmacopœia. He has now succeeded in eliminating this resinous matter, and supplies a true ginger-beer, not made with cayenne, as the commercial beverage sometimes is.

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2. The Edinburgh Medical Journal. Oliver and Boyd.
3. The Retrospect of Medicine. Edited by W. Braithwaite. Simpkin, Marshall, and Co.
4. Pharmaceutical Journal. Churchill.
5. The Lancet.
6. Medical Times and Gazette. Churchill.
7. The British Medical Journal.
8. The Asylum Journal of Mental Science. Churchill.
9. The Glasgow Medical Journal. Dunn and Wright.
10. The Dublin Medical Press and Circular.
11. The Westminster Review. Trübner.
12. Transactions of Obstetrical Society. London : Longmans.
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14. The Journal of Anatomy and Physiology. Macmillan.
15. The London Medical Record. Smith, Elder, and Co.
16. The Obstetrical Journal. London : J. and A. Churchill.
17. The Sanitary Record. London : Smith, Elder, and Co.
18. The Journal of Homœopathy. London.

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